

QUASI-RANDOM MATCHES: EVIDENCE FROM DUAL LABOR MARKETS

*Preliminary and Incomplete**

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Abstract

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. The key challenge when studying such questions is the non-random sorting of workers into jobs. We propose a novel identification strategy that exploits the *timing* of worker-firm matching. We isolate quasi-random variation in matches by interacting high-frequency information on the duration of contracts on the labor supply and transitory fluctuations in job creation on the labor demand side. We apply this method to address a central question in *dual labor markets*: how do different contract types – fixed-term or open-ended contracts – affect workers’ careers? Using Spanish Social Security records, we track workers since labor market entry and compute their cumulative experience in both contract types. We find that transitory variation in the opening of permanent contracts is highly predictive of individual promotion probabilities, and has long-lasting effects on earnings, employment, and the accumulation of experience in permanent positions.

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1 Introduction

A fast-growing literature studies how sorting into particular jobs, firms, or locations affects workers. Following [Abowd et al. \(1999\)](#), there has been much interest in the observation that pay premia vary across firms, the mechanisms that generate such variation ([Manning 2021](#), [Card et al. 2018](#)), and its implications ([Card et al. 2013](#)). A natural question then is whether jobs also differ in their *dynamic* implications – if workers learn more and enjoy faster earnings growth in some jobs while being “stuck” in others. Indeed, recent studies suggest that earnings growth varies systematically across firms ([Arellano-Bover and Saltiel 2021](#), [Pesola 2011](#)), regions ([Roca and Puga 2017](#)), and jobs ([Gathmann and Schönberg 2010](#); [Garcia-Louzao et al. 2021](#); [Kambourov and Manovskii 2009](#)).

The key challenge when studying such questions is the non-random sorting of workers into jobs. For example, firms paying higher wages might attract better applicants, and workers in urban labor markets might be different from those in rural areas. To address this selection problem, the literature has adopted a fixed effect strategy: by tracking workers across firms, researchers can decompose wages into time-constant differences between individuals (individual fixed effects) and match-specific components (such as firm fixed effects, as in [Abowd et al. 1999](#)). While this strategy is ubiquitous, there is an obvious tension: if workers or firms differ in their *level* of pay, they might also differ in wage *growth*, which would not be captured by the fixed effects.

In this paper we propose an alternative strategy that exploits the *timing* of worker-firm matching. Specifically, we isolate quasi-random variation in matches by interacting high-frequency information on (i) the duration of contracts on the supply side of the labor market, and (ii) transitory fluctuations in job creation on the demand side. We apply this method to address a central question in “dual” labor markets: how do different contract types – fixed-term or open-ended contracts – affect workers’ careers? A common concern is that fixed-term contracts may discourage firms from providing training or other investments to their workers ([Cabrales et al. 2017](#); [Albert et al. 2005](#)). While we focus on the consequences for workers, this problem has important aggregate implications, and the prevalence of fixed-term contracts are one suspected reason for low labor productivity in countries characterized by dual labor markets ([Cahuc et al. 2016](#)).

Our application focuses on Spain. With the highest rate of temporary employment in Europe of nearly 25% and as much as 90% of new contracts being fixed-term (in recent years before a major reform in 2022), the country provides an interesting context. Moreover, we can exploit rich matched employer-employee data from Social Security records that track workers over time and contain detailed information on the type and length of individual employment contracts.

We first provide evidence using a standard fixed effects approach, estimating an earnings equation that allows for time-constant differences between individuals and different rates of worker experience gained in fixed-term or open-ended contracts. Consistent with recent evidence by [Garcia-Louzao et al. \(2021\)](#), we find that earnings growth is higher in open-ended contracts: while earnings grow by 2.7 percent in fixed-term contracts, they grow by 3.6 percent per year in open-ended contracts. We also find that these patterns are highly non-linear; while wage growth does not vary with contract type for inexperienced workers, there is a sizable difference in returns for more experienced workers: for workers with ten years of experience, wage growth is 22% percent higher in open-ended than in fixed-term contracts. These patterns are broadly in line with recent evidence by [Garcia-Louzao et al. \(2021\)](#).

These patterns might reflect that fixed-term contracts slow skill acquisition and wage growth. However, they could also be due to returns varying across workers independent of contract type. A key piece of evidence to distinguish between these explanations is an event study graph to show “parallel pre-trends”. For example, [Card et al. \(2013\)](#) show that workers who switch from low- to higher-paying firms tend to experience similar wage growth as those that make the reverse switch, suggesting that worker-firm matching is sufficiently random in a dynamic sense. However, by implementing an equivalent type of test, we show that the parallel trends assumption does not hold in dual labor markets: workers who switch into an open-ended contract as opposed to another fixed-term contract experienced higher wage growth even *before* they entered their new contract. The difference is sizable: while the earnings of workers switching to an open-ended contract grow, on average, by 5% in the year before the switch, earnings growth is negligible for workers who switch into another fixed-term contracts instead. This gap remains large when controlling for a detailed set of worker characteristics. This observation suggests that matching workers to contract types is not random: the differences in wage growth between fixed-term and open-ended contracts might reflect heterogeneity between workers rather than differences in returns between contract types.

The selection of workers into contracts is therefore a more difficult selection problem than the selection into firms ([Card et al. 2013](#)) or regions ([Card et al. 2021](#)). We discuss several reasons why this might be the case. One factor is that the switch to open-ended contracts occurs more often within firms and is therefore based on more information than in the case of workers matching to other firms. Moreover, switching into an open-ended contract within a firm can be a form of promotion; and promotions depend, of course, on the recent performance of the worker. Finally, higher-ability workers are more likely to be matched to better fixed-term contracts, i.e., they might be able to find actual stepping-stones. They would therefore display differential pre-trends even before switching to a permanent position.

Our paper, therefore, adds to two distinct strands of literature. On the methodological side, we relate to recent papers that extend the standard two-way fixed effects specification to account for more complicated forms of selection. For example, [Roca and Puga \(2017\)](#) evaluate returns to experience heterogeneity based on city size. Their approach explores both static and dynamic advantages, particularly, they consider heterogeneity of city gains across workers by interacting unobserved innate ability measured by individual fixed-effects and city-size specific experience. Using a similar approach, [Arellano-Bover and Saltiel \(2021\)](#) show that returns to experience vary across firm types. Applying a clustering methodology, they are able to classify firms into *skill-learning* classes which they show, are not predicted by firms’ observable characteristics.

Compared to these papers, we follow a very different strategy: rather than enriching the fixed effects specification to account for specific forms of heterogeneity and dynamic selection, we isolate quasi-random variation in the matching between workers and firms using an instrumental variable strategy. That is, rather than trying to control for the selection process, we aim to circumvent it. Specifically, we interact individual variation in the expiration date of fixed-term contracts with transitory fluctuations in the opening of new open-ended jobs over time to isolate exogenous variation in contract type.

Conceptually, our strategy is similar to studies that analyze the effects of labor market conditions at entry on worker careers (“graduating into a recession”), in particular, recent work by [Arellano-Bover \(2020\)](#) on the selection into different firms. However, rather than exploiting yearly variation in labor market entry, we exploit high-frequency information on the duration of contracts. Specifically, exploiting the precision of administrative employment records, we are able to match the precise month when the individual’s contract is about to end with transitory variation in job openings at the regional level. Our approach faces the usual challenges in establishing instrument relevance and validity. The upside however is that we do not have to specify the functional form of individual heterogeneity and dynamic selection.

We first establish the instrument’s relevance, showing that the (leave-out) sum of new open-ended contracts is highly predictive for a worker to switch from a fixed-term into an open-ended contract. We then provide evidence to support the instrument independence assumption and exclusion restriction. Instrument independence would imply that facing more open-ended job openings (relative to trend) in the month a contract ends is as-good-as random for the worker. To support this assumption, we show that our instrument is indeed broadly uncorrelated with worker characteristics. However, the exclusion restriction is unlikely to hold without further adjustments. The number of new open-ended contracts (our instrument) does of course correlate with general business cycle conditions, so it is not obvious whether a worker enjoys higher wage growth because she

started in an open-ended contract or because the economic conditions in this period were generally favorable, affecting wage growth conditional on the contract type. The objective therefore becomes to control for general economic trends while exploiting the exact timing of when an individual switched jobs, i.e., we exploit high-frequency variation in the types of contracts that are available while controlling for low(er)-frequency business cycle variation.

To the best of our knowledge, we are the first to exploit this source of exogenous variation to deal with the endogenous sorting of workers into jobs. We argue that it is applicable in many settings. While administrative panel data are not without problems, they offer highly precise (typically, daily) information on the duration of contracts as this information is directly relevant for taxes and social security contributions. Our approach exploits therefore a comparative advantage of administrative data (their high frequency), similarly as the fixed effects approach exploits another (their scale).

As a second contribution, we add to the active literature on dual labor markets ([Bentolila et al. 2020](#)). The two-tier segmentation that characterizes many European labor markets is the result of a series of reforms that started in the 1980s and intended to tackle high structural unemployment. Fueled by regulations that aimed to introduce more hiring flexibility, fixed-term contracts became widespread. While these low-firing-cost contracts may, in theory, help a fraction of workers avoid long periods of unemployment, they may also come at the expense of lower human capital accumulation and poor progression toward better jobs.

Indeed, previous studies have shown that workers in temporary positions receive less firm-provided training ([Cabrales et al. 2017](#); [Bratti et al. 2021](#)). With asymmetric on-the-job learning opportunities and uncertain conversion to permanent positions, long histories of recurrent fixed-term spells can perpetuate workers in low-wage-growth trajectories. While fixed-term contracts may serve as stepping-stones to more stable jobs, the favorable evidence mostly corresponds to countries with low firing costs for fixed and open-ended positions alike ([Bentolila et al., 2020](#)). For countries such as Spain and Italy, where not only the share of temporary jobs is higher but also the gaps in employment protection by type contract are large, previous evidence suggests that these contracts more often result in “dead ends” ([Gagliarducci 2005](#); [Güell and Petrongolo 2007](#); [García-Pérez and Muñoz-Bullón 2011](#); [Garcia-Louzao et al. 2021](#)).

The paper is organized as follows: Section 2 provides a background of the institutional framework, Section 3 introduces the main data source, Section 4 provides a characterization of dualism in Spain and preliminary results of a mincerian approach, Sections 5 and 6 discuss the main sources of endogeneity and our identification strategy, respectively and Section 7 analyses the effect of upgrade promotion in workers’ career trajectory by

evaluating a series of labor market outcomes.

2 Institutional framework

In the aftermath of the dictatorship, Spain's institutions underwent major changes, including reforming its labor market legislation. Before 1976, labor laws in Spain were liberal [Toharia \(2002\)](#), as most labor contracts required only the acceptance of both employers and employees. The first step toward modernization was Law 16/1976 ¹. Under this law, however, all contracts were considered full-time permanent, which represented a major restriction for industries with a large seasonal variation.

Initiating the dualism of the Spanish labor market, Law 32/1984 established the co-existence of permanent and temporary contracts, still affecting the labor market today. With this reform, firms whose activities are not seasonal could sign temporary contracts with any worker. Therefore, firms may open permanent vacancies with a high severance payment or temporary vacancies with a smaller severance payment. The reform did not alter any of the conditions for permanent contracts, which made temporary contracts even more appealing for firms ([García-Pérez et al. 2019](#), [Aguirregabiria and Alonso-Borrego 2014](#)). Not surprisingly, temporary jobs accounted for a large percentage of new jobs created during the economic expansion period.

As a response, in 1994 temporary employment contracts were limited to seasonal work, and dismissal conditions for permanent employees were relaxed. In practice, however, employers hired temporary workers for all jobs, not just seasonal ones ([García-Pérez et al., 2019](#)). This perceived ineffectiveness of the 1994 reform led to additional reforms in 1997 and 2001. The changes created a new permanent contract with a smaller severance payment of 33 days than the 45 in the previous reforms —this new contract was aimed at the young and those older than 45 and people with disabilities. ²

It was not until the reform of 2012 that there was a significant reduction in the costs of hiring permanent workers. The compensation at the termination of the temporary contract was increased, reducing the gap between the dismissal costs of workers with permanent and temporary contracts. In addition, the reform eliminated interim wages in judicial processes. A new OEC was introduced for firms below 50 employees, entailing no severance pay during an extended probationary period of one year. But fixed-term contracts still accounted for more than 20% of all employees.

¹Ley 16/1976 de 8 de Abril de Relaciones Laborales.

²The reform of 2001 also included women hired in sectors where they are underrepresented and long-term unemployed.

Various reforms have been implemented in the last 30 years to decrease labor market dualism while preserving flexibility in hiring. The proportion of workers in a temporary contract has also decreased during that time. Still, many workers begin their working life on a temporary contract and experience a long sequence of unstable jobs. One major concern is that this lack of job stability has adverse consequences for the accumulation of human capital, fertility and wages.

3 Data

Our main data source combines the 2006-2017 waves of the Continuous Sample of Working lives (*Muestra Continua de Vidas Laborales* or MCVL). The microdata from the MCVL constitutes a 4% non-stratified random sample of Spain's Social Security administrative records. The sample allows tracking the full working history of individuals back to 1967 and the monthly earnings since 1980. Once an individual with an ongoing relationship with Social Security is included in the sample, it remains in all future waves.³ Furthermore, every year, those individuals that are no longer affiliated with Social Security are replaced with new workers (along with their whole past labor history). This updating exercise ensures that the sample remains representative.

Several features make this rich dataset optimal for our analysis. A key advantage of the MCVL is its high-frequency records. This enables us to build the estimation sample at a monthly level. Since we have information on each spell's entry and exit date, we are able to compute the exact days an employee worked. Whenever there is an overlap of spells, we preserve the job characteristics of the main job: i.e., the largest spell of the month. We are then able to build a reliable measure of tenure and work experience with a clear distinction between the experience accumulated in fixed-term and open-ended contracts.

Furthermore, the Social Security records are matched with annual information from the municipal population registry (*Padrón Continuo Municipal*) and income tax records from 2006 onward. The former allows us to expand on workers' demographic characteristics, and the latter on additional worker and firm characteristics. We observe the date of birth, gender, educational attainment, and country of birth for each worker. While we do not observe occupation directly, we sort workers into five occupational-skill groups that we define based on ten occupational contribution categories that employers have to report to Social Security. In principle, these refer to the skill required for a particular job and not necessarily the skills acquired by the worker. Still, they are closely related to the required formal education to execute a particular job.

³Employees, self-employed individuals, pensioners, and people receiving unemployment benefits are included in this category.

At the firm level, we observe the province where the firm is located and its size from 2006. Strictly speaking, while a firm can have more than one establishment in different provinces, we treat each establishment as a separate firm. Additionally, for each job, we observe the sector of the economic activity at the two-digit level, the type of contract (permanent or fixed term, full-time or part-time), and whether the worker is self-employed, or a private or public sector employee.

Given that the social security taxable base is bottom and top coded,⁴ we compute monthly real⁵ earnings from tax records whenever available, which are not subject to censorship. Combining data from several waves allows us to reconstruct the history of tax records which, unlike social security records, do not contain the worker's retrospective history. In earlier years, we used information from Social Security. Likewise, given that the Autonomous Communities of Navarre and Basque Country collect income taxes independently from the National Government, we only observe social security records for workers of those regions. The accurate information on the length of each spell works as a proxy on days worked during each month, enabling us to compute daily wages.

3.1 Sample restrictions

Our study evaluates the 1998-2017 period. Although we can trace each worker's earnings trajectory back to 1980's, information on type of contract is reliable from 1998 onwards. In general, we focus on workers that are between 18-49 years old. We restrict the analysis to workers that are registered in the general regime of social security or the special regime for agrarian, sea workers, and mining. This excludes autonomous workers. Since they are not employees and therefore do not hold a contract, they are not part of our study.

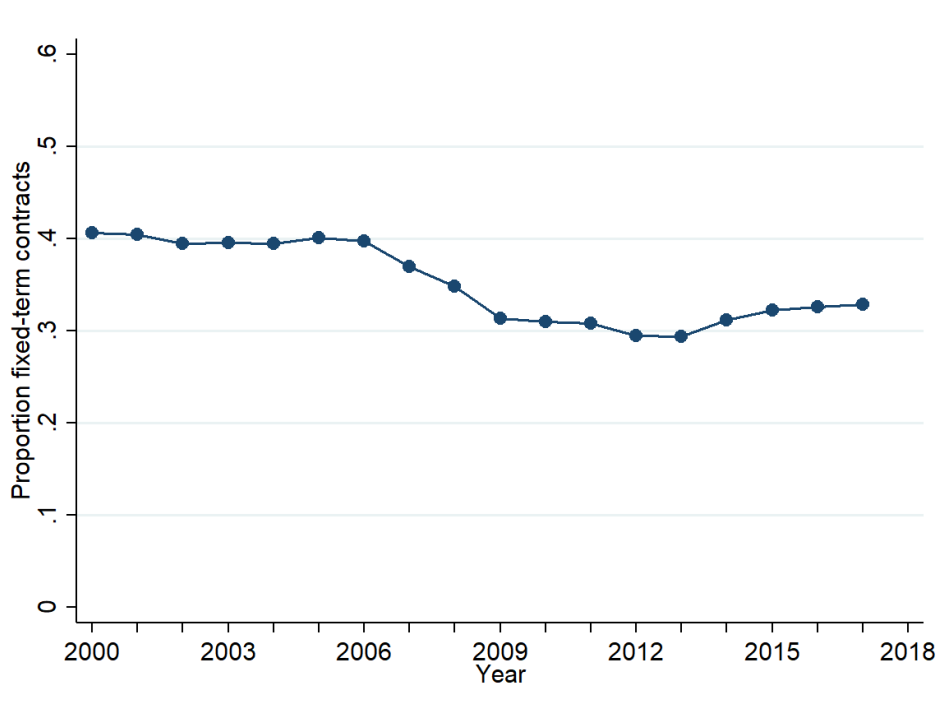
In our main specification we only consider private sector workers, as the contract duration of public sector employees is highly regulated and centralized, as well as the promotion to permanent positions relies on a special process.⁶ However, whenever is the case, our measure of experience does take into account the time that a private employee previously worked in the public sector, either in a fixed or a permanent contract. Regionally, we exclude information from Ceuta and Melilla, for which the sample of workers is very small. Thus, we work with data from 50 provinces.

⁴The upper and lower bounds are specified by sector and updated every year.

⁵Nominal wages are deflated using the 2009 Consumer Price Index.

⁶Workers in the public sector are usually required to approve specific exams and fulfill special requirements to get a permanent position. This process is quite different from the promotion path of private sector workers.

Figure 1: Proportion of workers in fixed-term contracts, by year



Notes: Proportion of workers under a fixed-term from 2000 to 2017.

4 Descriptive evidence

Around one third of employees worked under a fixed-term contract in recent decades in Spain. Despite a decline in the share of temporary workers in the aftermath of the Great Recession (Figure 1), their share is still very high compared to most European countries.⁷ The reduction in the proportion of fixed-term contracts reflects to a great extent the decrease in hiring after the financial crisis. The construction sector, which concentrated a large share of temporary workers, was one of the hardest hit. Likewise, young workers' unemployment increased dramatically and remained high for many years, spiking from around 22.3% in 2004 to 44.5% in 2016. This situation also affected the age distribution of temporary workers. As shown in Table 1, the share of fixed-term contract workers under 24 years almost halved, from 20.7% in 2004 to 11.2% in 2016.

As discussed previously, the high dualism in the Spanish labour market implies that rather than working as stepping-stones, a large proportion of fixed-term contracts are dead-ends. While this problem is more severe for low-skilled occupations, it cannot be neglected at the top of the distribution. As shown in Table 1, the share of high-skilled occupations jobs among temporary contracts has steadily increased. In terms of other worker and job characteristics, these contracts are equally spread among women and men. While most of these contracts correspond to full-time positions, the proportion of

⁷In 2019, more than 25% of Spanish workers were on a temporary contract. See Figure A3.

part-time jobs under this modality has increased substantially, representing one third of these jobs by 2016.

Table 1: Characteristics of workers in fixed-term contracts

| | 2004 | 2008 | 2012 | 2016 |
|---------------------------------|-------|-------|-------|-------|
| Age group | | | | |
| <24 | 0.207 | 0.174 | 0.116 | 0.112 |
| 24-35 | 0.487 | 0.458 | 0.433 | 0.388 |
| 36-50 | 0.262 | 0.316 | 0.373 | 0.400 |
| >50 | 0.044 | 0.052 | 0.079 | 0.099 |
| Foreign | 0.137 | 0.234 | 0.205 | 0.176 |
| Female | 0.429 | 0.457 | 0.500 | 0.489 |
| Part-time | 0.192 | 0.198 | 0.308 | 0.317 |
| Occupations | | | | |
| Very high skilled occupations | 0.050 | 0.059 | 0.083 | 0.080 |
| High-skilled occupations | 0.070 | 0.081 | 0.100 | 0.095 |
| Medium high skilled occupations | 0.117 | 0.126 | 0.142 | 0.134 |
| Medium low skilled occupations | 0.475 | 0.479 | 0.431 | 0.419 |
| Low-skilled occupations | 0.288 | 0.255 | 0.244 | 0.272 |

Notes: Characteristics of workers employed under fixed-term contracts.
Source: MCVL 2006-2017.

For comparability with previous studies on returns to experience (Roca and Puga, 2017; Garcia-Louzao et al., 2021; Arellano-Bover and Saltiel, 2021), we begin our descriptive analysis by estimating the contribution of contract-specific experience to earnings growth using a classic fixed-effects Mincerian equation. We account for differential returns by modeling explicitly combinations of experience accumulated in fixed-term and open-ended contracts. We estimate the following equation by OLS:

$$\ln w_{irt} = \exp_{it}^{FT}(\beta_1 + \exp_{it}\beta_2) + \exp_{it}^{OEC}(\beta_3 + \exp_{it}\beta_4) + X_{it}'\boldsymbol{\Omega} + \sigma_r + \psi_t + \varepsilon_{irt}, \quad (1)$$

where \exp_{it}^{FT} and \exp_{it}^{OEC} denote the worker's experience accumulated until period t in fixed-term and in open-ended contracts, respectively. The variable \exp_{it} is the total experience of individual i up to period t . X_{it} is a vector of time-varying individual and job characteristics, including gender and occupation-skill group interacted with educational attainment, sector fixed-effects, age, age squared, and an interaction of tenure with a fixed-term contract indicator, σ_r is a region fixed effect, ψ_t is a year-month fixed-effect, and ε_{ict} is the error term.

Instead of the typical quadratic form of homogeneous returns to experience, equation (1) considers the product between overall experience and contract-specific experience. This interaction captures the fact that the moment at which workers accumulate experience in each type of contract matters. In other words, the returns to an extra year

Table 2: Wage growth in fixed-term and open-ended contracts

| | (1) | (2) | (3) |
|-----------------------------|-------------------------|-------------------------|-------------------------|
| | ln earnings | | |
| exp | 0.0508*** (0.000528) | | |
| $exp^2/1000$ | -1.314*** (0.0323) | | |
| exp_{FT} | | 0.0637*** (0.000795) | 0.0794*** (0.00103) |
| exp_{OEC} | | 0.0558*** (0.000613) | 0.0706*** (0.000712) |
| $exp \times exp_{FT}/1000$ | | -3.373*** (0.0634) | -3.312*** (0.0552) |
| $exp \times exp_{OEC}/1000$ | | -1.049*** (0.0389) | -1.446*** (0.0311) |
| Obs. | 16266496 | 16266496 | 16255262 |
| R^2 | 0.475 | 0.478 | 0.754 |
| Controls | Yes | Yes | Yes |
| Ind. FE | No | No | Yes |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: exp , exp_{FT} , and exp_{OEC} account for experience, experience in fixed-term, and experience in open-ended contracts, respectively. Controls include gender and occupation-skill group interactions on education attainment, sector, region and time fixed-effects, age, age squared, and interactions of tenure with an indicator for a fixed-term contract. Errors are clustered at the worker level.

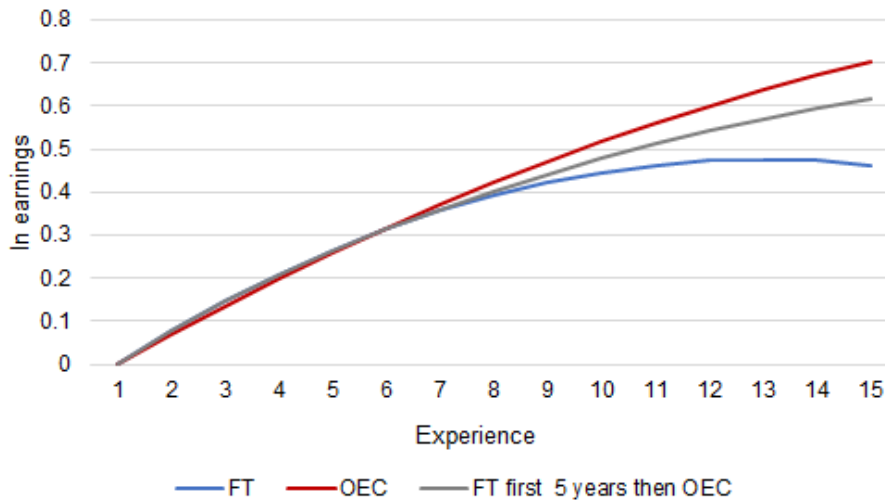
of lower-quality experience at the very beginning of the career may be different to the returns at mid-career.

For reference, the specification of Column (1) in Table 2 assumes that, on average, both contracts give the same return. According to the estimated results, one extra year of experience is associated with an increase in individual earnings of 2.5% for workers with ten years of experience. Column (2) breaks down experience by the type of contract where it was accumulated. While the coefficients on linear experience are similar for both contract types, the main differences in workers' trajectories arise from the interaction terms, capturing precisely the above-mentioned timing effect: while the first years of experience in open-ended or fixed-term contracts yields similar wage returns, the growth rate for those in fixed-term contracts is lower in subsequent years. For a worker with ten years of experience, an additional year on a fixed-term contract translates into a 3.0% increase in earnings. In contrast, an additional year in an open-ended contract is

associated with a 4.5% surge.

Although this specification acknowledges that the value of accumulated experience in each type of contract might differ, it ignores the potential sorting of workers into each type of position. For instance, if high-ability workers are over-represented in open-ended positions, the coefficients of Column (2) might reflect that more able workers tend to enjoy higher wages irrespective of contract type. Previous work has addressed this concern by including worker fixed-effects, as in Column (3). The worker-fixed-effect slightly attenuates the gap between fixed-term and open-ended contract returns, but the overall pattern remains the same. For a worker with ten years of experience, an additional year in a fixed-term position is associated with a wage growth of 4.6% as compared to 5.6% if this experience was accumulated in a permanent contract.

Figure 2: Heterogeneous returns to experience by contract type



Notes: Fitted values based on experience coefficients from Column (3) in Table 2.

Based on these results, Figure A7 illustrates the earnings trajectory for workers who accumulate experience in a fixed-term, open-ended contract or a combination of both. While wage growth is almost equal over the first years, after six years the gap in favor of open-ended positions rapidly widens. After ten years, the earnings of a worker employed only in open-ended contracts differ from those who only accumulated fixed-term experience by 21%. As we show next, these estimates have however no causal interpretation, as they reflect that more able workers are (i) more likely to enter an open-ended contract and (ii) enjoy faster earnings growth irrespective of contract type, a form of selection that is not captured by the fixed-effects approach.

5 Selection into permanent positions

The results so far provided suggestive evidence about the differential value of experience that each of these contracts produce: with fewer on-the-job-training opportunities (Cabrales et al., 2017), a temporary contract in a country with high dualism might result in less skill accumulation.

However, a worker fixed-effects specification only captures part of the endogeneity problem arising from contract sorting. Specifically, worker fixed effects would not capture the dynamic dimension of this problem: i) the timing of promotion matters, and high-ability workers are more likely to enter a permanent position earlier. Once workers are employed in an open-ended position, the rate of human capital accumulation is expected to be higher. Thus, skill acquisition and unobserved ability will evolve differently between the two contracts. ii) signaling: once a worker enters an open-ended position, the market might automatically perceive her as a higher-ability candidate, therefore, she would automatically face better job opportunities than a similar candidate in a fixed-term position iii) worker-firm-match quality, even if both low and high-ability workers started in a fixed-term position, which is usually the case in our context,⁸ higher-ability workers are more likely to be matched with better quality fixed-term contracts: i.e. they might be able to find actual “stepping-stones”. If this was the case, even before one of these workers switches into a permanent position, they would display differential pre-trends. This is precisely the problem that we aim to address.

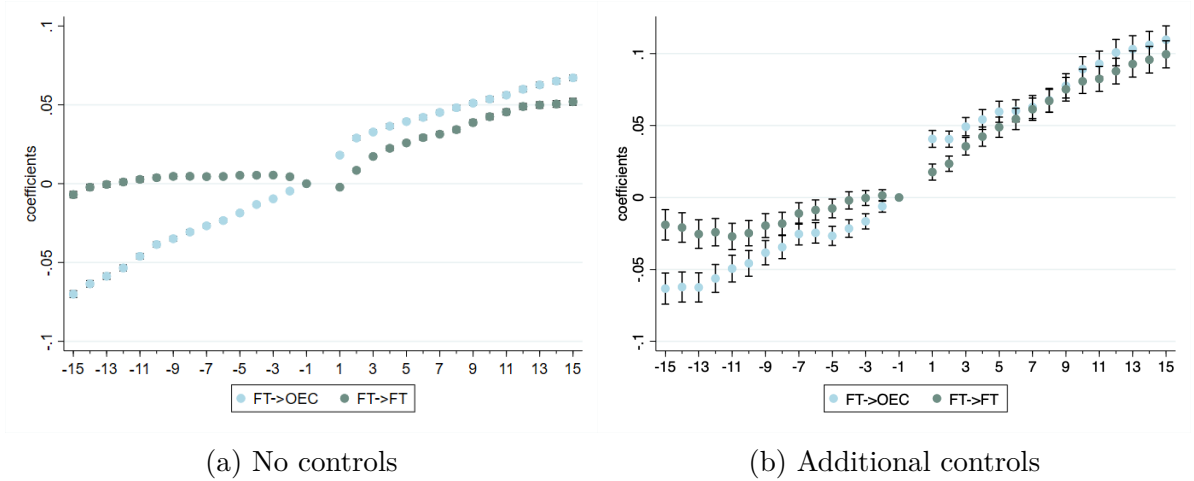
Ideally, we would like workers to be randomly assigned to different contract types at different career points when estimating return to experience under open-ended and fixed-term contracts. Nevertheless, workers’ chances of receiving an open-ended contract early in their careers correlate with their abilities and human capital, affecting their experience accumulation. Thus, a simple fixed-effects strategy could lead to biased results, even on a comparable sample of workers.

To confirm the problem, we examine whether workers with open-ended and fixed-term contracts follow parallel earnings paths before they are promoted using an event-study design. We therefore trace out the dynamic trajectory of workers’ earnings around the timing of job switches. For each worker in the data, we denote the precise month in which the individual ends a temporary contract by $t = 0$,⁹ and index future and past months relative to that moment. After the contract ends, we categorize workers based on their future type of contract. A worker i promoted to an open-ended contract is called transitioned worker ($T_i = 1$). If she is not promoted will have a ‘no-transition’ status

⁸Nearly 90% of new jobs in Spain are fixed-term.

⁹The term “new contract” can refer to a worker starting a fixed-term contract in a new firm or moving from a fixed-term contract into an open-ended position within the same firm.

Figure 3: Earnings consequences from transitioning to OEC or FTC



Notes: The base category is $t=-1$. Panel (a) Controls for the full set of time and age dummies. Panel (b) includes additional interactions of event time with education and sector FE. Errors are clustered at the worker level. The coefficient from event period 0 is omitted from each graph given that not all workers work the whole last month.

($T_i = 0$). Our baseline specification considers a balanced panel of workers whom we observe fifteen periods (months) before and after the event,¹⁰ so the event time t runs from -15 to $+15$. We denote by y_{ist} the log earnings of individual i , in year-month s and at event time t , from which we run the following regression:

$$\begin{aligned}
 y_{ist} = & \sum_{j \neq -1} \alpha_j^T \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[T_i = 1] + \sum_{j \neq -1} \alpha_j^{NT} \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[T_i = 0] \\
 & + \sum_k \beta_k \cdot \mathbf{I}[k = age_{is}] + \sum_p \gamma_p \cdot \mathbf{I}[p = s] + \nu_{ist},
 \end{aligned} \tag{2}$$

where we include a full set of event time dummies (first term on the right-hand side), age dummies (second term), and year x month dummies (third term), we omit the event time dummy at $t = -1$, implying that the event time coefficients measure the impact of moving into a new contract relative to earnings just before the termination of the previous fixed-term contract. If we would not include age and time dummies, the estimated event coefficients ($\alpha_j^T, \alpha_j^{NT}$) would correspond simply to the mean value of the outcome at event time t , relative to the time before changing contract. By including a full set of age dummies, we control non-parametrically for underlying life-cycle trends. Additionally, including a full set of time dummies, we control non-parametrically for time trends such as business cycle variation. Including age dummies in the comparison is important because workers in open-ended positions tend to be older than workers who remain in temporary positions.

¹⁰Periods may differ from months if workers have a non-employment spell within those fifteen months before or after. Due to sample restrictions, this is ruled out on the pre-period.

Results are presented in Figure 3. We compare the earnings trajectories of workers promoted into an open-ended contract (FT→OEC) and those who started another temporary contracts (FT→FT). Panel (a) controls for the full set of time and age dummies discussed above. Additionally, Panel (b) also accounts for interactions between event time and worker’s education and sector, thereby accounting for observable differences in earnings growth. The estimates remain unchanged if we included worker fixed effects, as we consider a balanced sample of workers and the estimates represent the earnings growth of those workers compared to the base period (i.e., worker fixed effects are netted out already).

We would expect that workers face a differential earnings path after event period 0, as temporary contracts may be subject either to earnings penalties or premia (Albanese and Gallo 2020; Kahn 2016), and because returns to experience depend on contract type (previous section). However, we observe that earnings evolve very differently even *before* workers start their new contract: those workers who subsequently switch into open-ended contracts enjoy *much* faster earnings growth than those who do not, even while both groups are still in fixed-term contracts. The finding of higher wage returns among workers with more open-ended work experience therefore partially reflects this difference in worker selection.

6 Identification

In order to deal with the endogeneity of promotions into permanent positions, we propose an instrumental variable strategy. As exogenous source of variation, we combine individual variation in the expiration date of a fixed-term contract and transitory fluctuations in the opening of new open-ended jobs over time and space. Workers face a positive shock if there is an abnormal increase in permanent openings in the labor market just before their contract expires. This affects promotion probabilities in two ways: the most direct channel, workers face a tighter labor market with more opportunities of landing on a permanent job outside the firm just because its availability is higher. The second one, under these conditions, other workers might switch to a job in a new firm, creating vacancies that could be filled by promoting fixed-term workers whose contract is about to end.

Exploiting the high-frequency of our data, we are able to match precisely the month when the individual contract is about to end with the job openings at the regional level. We argue that facing more job openings precisely in the month that a contract was about to end is as good as random for the worker.

Specifically, using a leave-one-out approach, we estimate the following first-stage equation:

$$p_{it} = \sum_{k=-12}^{12} \alpha_k \log OEC_{-i,t+k} + X_{it}\theta + \epsilon_{it}, \quad (3)$$

where p_{it} indicates whether the worker is promoted to an open-ended contract in $t+1$. The variable $\log OEC_{-i,t+k}$ is constructed as the sum of all new open-ended positions in period t , leaving-out individual i herself. We therefore allow for promotions to depend on the total number of new open-ended contracts in period t , and leads and lags of this variable excluding individual's i promotion in the calculation. The first lead, $\log OEC_{-i,t+1}$, is our instrumental variable. Under our identification assumptions, we would expect the effect of this first lead, captured by coefficient α_1 , to be the strongest predictor of individual's probability to switch into a permanent position. The other coefficients α_k for $k \neq 1$ should be smaller in magnitude, but might be non-zero, as they capture general business cycle conditions that might not be fully captured by α_1 .

The inclusion of other leads and lags therefore serves two purposes. First, to illustrate that transitory fluctuations matter if they hit a worker in exactly the month in which her previous contract runs out, i.e. to show that the first lag has strong predictive power even conditional on a complete set of other leads and lags (instrument relevance). Second, these other leads and lags control for general business cycle conditions (which would violate the exclusion restriction). To further partial-out the effect of the business cycle and seasonal variations in job openings, we add an extensive set of controls including leads and lags of the total number of new jobs including those in fixed-term contracts, year, month, province, and sector fixed effects. At the individual level, we also control for gender, overall experience, experience squared, and interactions of age categories with education attainment.

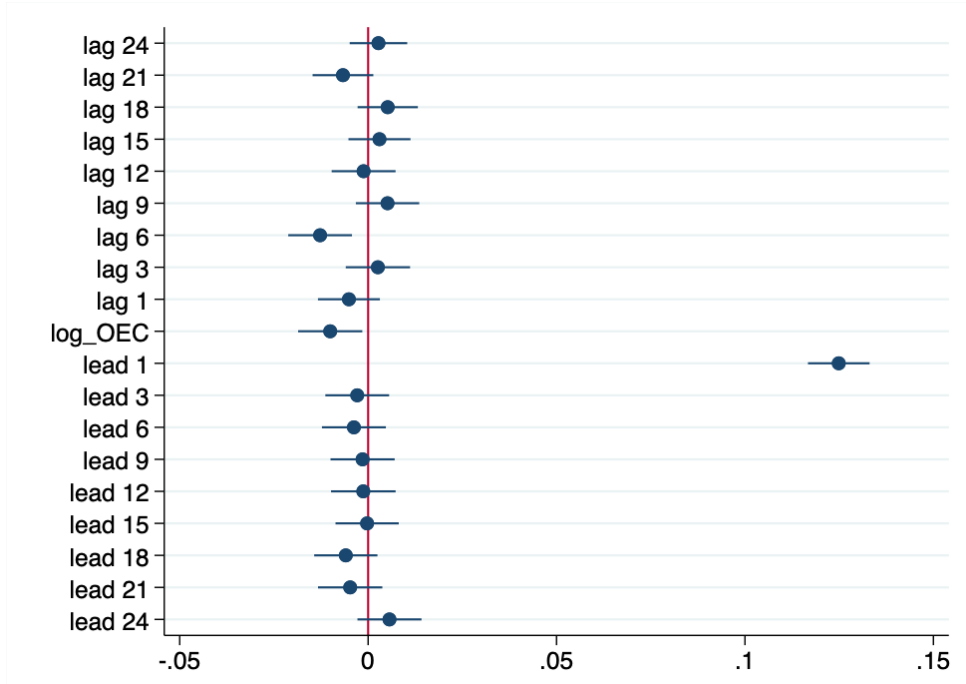
The results from this regression are presented in Figure 4.¹¹ As expected, the effect of the first lead of new permanent positions stands-out strongly. Consistent with our identification strategy, we find that the openings of new open-ended contracts when the worker's contract expires is the strongest predictor of the probability of finding a permanent position immediately after. Moreover, the absence of strong correlations with the rest of the leads and lags indicates that the instrument is capturing the effect of transitory shocks on job market matches, as opposed to general business cycle conditions.

Figure 4 refers to leads and lags in the number of new open positions on the *regional* level. We can apply the same logic to instead exploit new openings of permanent positions at the regional and industry level, which might be more consequential for an individual's labor market chances. As shown in Figure A4 in the Appendix, we find similar patterns

¹¹The regression estimates for the baseline and alternative specifications are reported in Tables 7, 8, 9, 10.

in these alternative specifications.

Figure 4: The effect of new open-ended contracts on promotion probabilities



Notes: Sample: Workers in the last month of a fixed-term contract of least 0.8 years of tenure but less than 1.2 years of tenure. Coefficients of the probability of being promoted to a open-ended contract in $t + 1$ on leads and lags of the log of new open ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

7 Results: Reduced-form evidence

Labor market dualism may impact workers trajectories in several dimensions. Previously, we showed that regional variations in the opening of permanent contracts affect promotion probabilities. In a reduced-form approach, this section examines how improved upgrade to permanent positions opportunities affect workers' labor market outcomes in the short and long-term. Restricting the sample to those workers holding contracts that are about to end, we estimate the following equation:

$$y_{it} = \sum_{k=-24}^{24} \alpha_k \log OEC_{-i,t+k} + \sum_{k=-24}^{24} \gamma_k \log TNC_{-i,t+k} + X_{it} \theta + \epsilon_{it}, \quad (4)$$

where y_{it} the worker's i outcome in period t . Each outcome is studied up to 60 months before and after a fixed-term contract expiration, which allows us to explore long-term effects and evaluate the existence of pre-trends. We include 24 leads and lags of the \log

of new open-ended contracts ($\log OEC$) relative to the last month of the worker’s current fixed-term contract. In order to control for business cycle variation and job creation seasonality, we also include the same number of leads and lags of the \log total number of new contracts denoted by ($\log TNC$).

We can go further and control for business cycle variation more aggressively by additionally controlling for the aggregate leave-one-out average of the outcomes, $\bar{Y}_{-i,t}$, as in

$$y_{it} = \sum_{k=-24}^{24} \alpha_k \log OEC_{-i,t+k} + \sum_{k=-24}^{24} \gamma_k \log TNC_{-i,t+k} + \delta \bar{Y}_{-i,t} + X_{it}\theta + \epsilon_{it}, \quad (5)$$

We construct $\bar{Y}_{-i,t}$, based on the full sample of workers, irrespective of the timing of their contract expiration date. This should further ensure that we keep economic conditions constant such that our instrument only captures atypical variation in open-ended positions availability, uncorrelated with business-cycle trends. Finally, we add individual and regional controls including year, month, province, and sector fixed effects, overall experience, experience squared, gender, and interactions of age categories with education attainment.

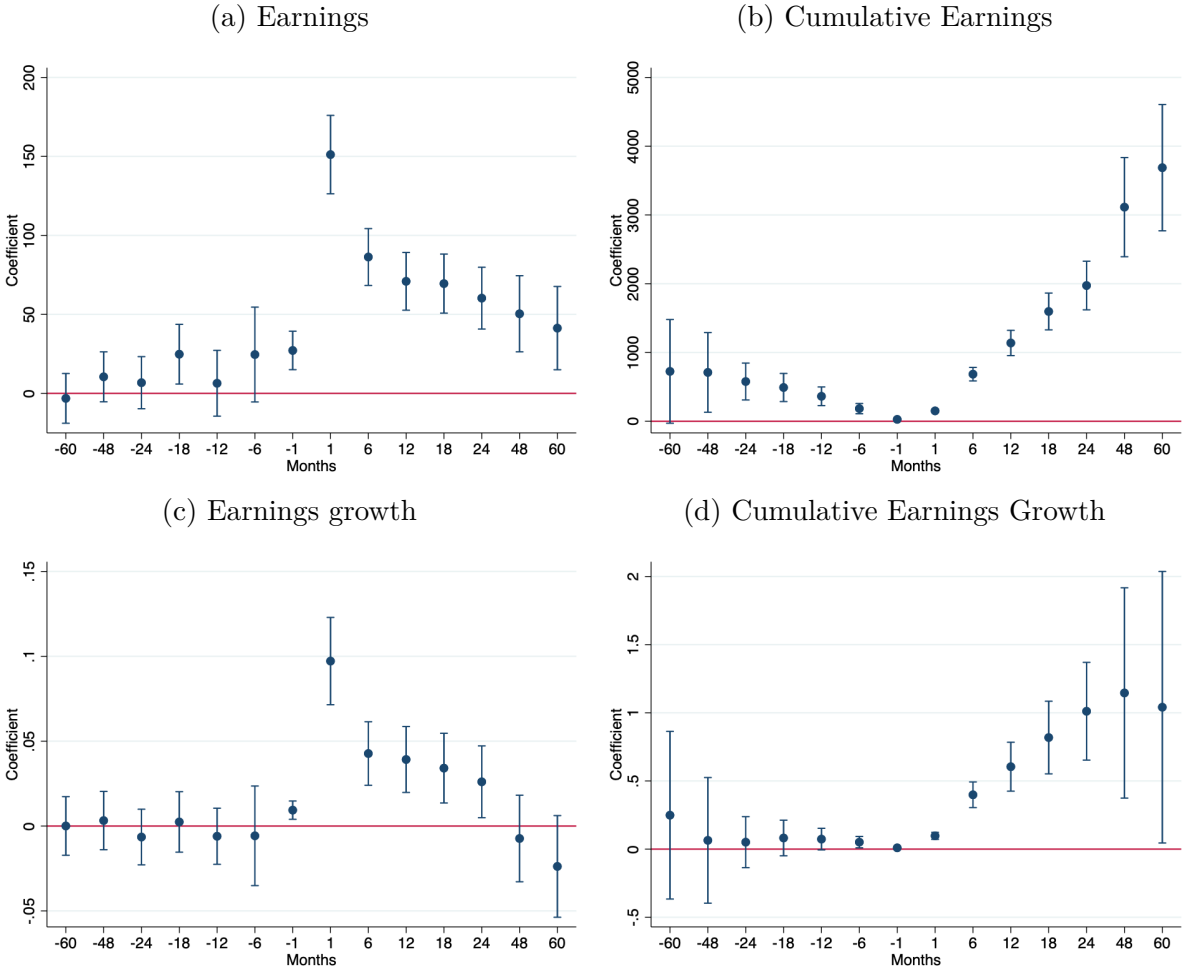
We consider four earnings-related outcomes. First, earnings levels which we construct by adding up the monthly labour-income m for each year. Cumulative earnings, which are the sum of workers’ earnings from the expiration of the fixed-term contract up to period t . Analogously, we construct earnings growth and cumulative earnings growth as the ratio between each variable at t and the monthly earnings at the baseline period 0: i.e. during the last month of the contract before expiring. Thus, the coefficients capture the effect on workers’ outcomes compared to the last contract they held before switching to a new (fixed-term or open-ended) position. In terms of employment we evaluate: employment status, the probability of being employed in an open-ended contract, and cumulative experience in open ended contracts measure in months. Additionally, we explore mobility responses.

7.1 Earnings

Figure A2 presents the long-term effects on workers’ earnings of the transitory increase in open-ended vacancies *just* at the time of the worker’s expiration date. We present the coefficient associated to the first lead of $\log OEC_{-i,t+1}$, α_1 , which we use as our source of exogenous variation. As shown in panel (a), we find a significant positive effect on worker’s earnings, which is more pronounced in the first year after the contract change. While the effect is persistent over time, we observe smaller magnitudes as time goes by. This reduction is mechanic to some extent. A fraction of workers who were *unlucky* at

$t=0$ and remained in a fixed-term contract, will eventually get promoted after a few years such that the gap with respect to those promoted at $t=0$ becomes smaller, explaining the observed effects. Workers who are more likely to be promoted also experience a significant increase in cumulative earnings (panel b) which captures both higher wages as well as more stable employment trajectories. Moreover, panel c) illustrates a positive effect on earnings growth concentrated over the first years, consistent with the same upgrade dynamics that we mentioned before.

Figure 5: Effect of OEC regional shock on earnings

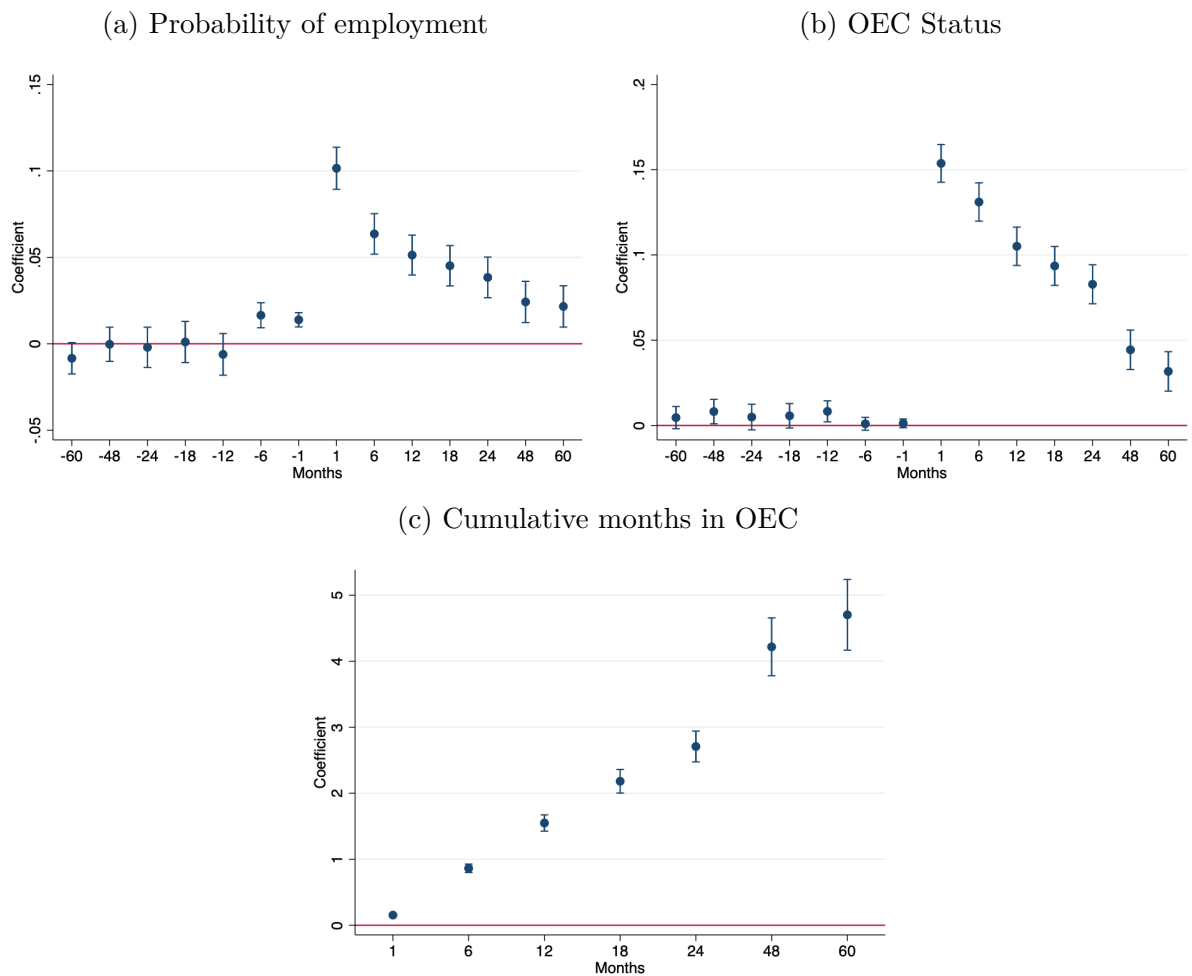


Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t , for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

7.2 Employment and Mobility

In terms of employment, our results suggest that upgrade to a permanent position places workers in a stable career path. As illustrated in Figure 6a, we find that the effect of better opportunities to switch into an open-ended contract translates into a higher employment probability even after 2 years of promotion. As expected, once workers start a job in an permanent contract, they are unlikely to return to a fixed-term position. Moreover, workers seem to be considerably less likely to change sector and slightly less prone to move to another region, as depicted in Figure 7.

Figure 6: Effect of OEC regional shock on employment



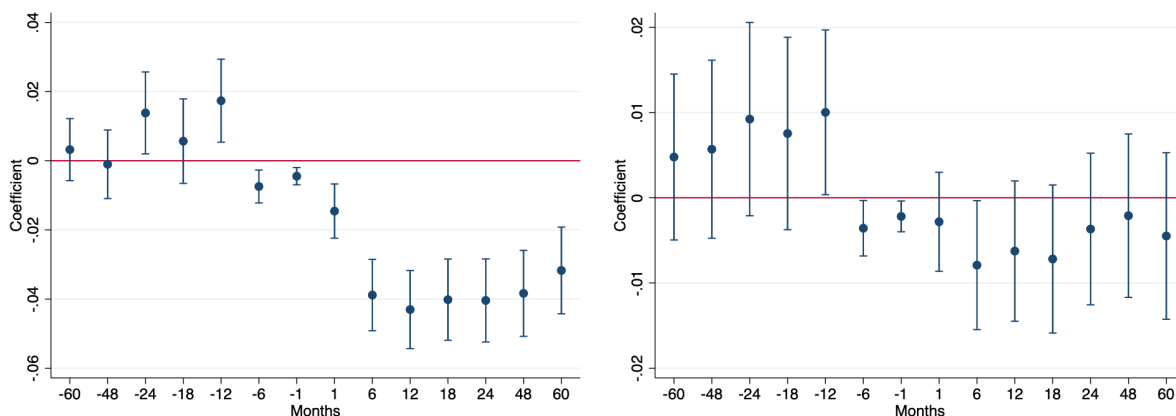
Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t , for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

Figure 7: Effect of OEC regional shock on workers' mobility

(controlling for total new contracts)

(a) Probability to change sector

(b) Probability to change region



Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t , for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

8 Conclusions

TBA

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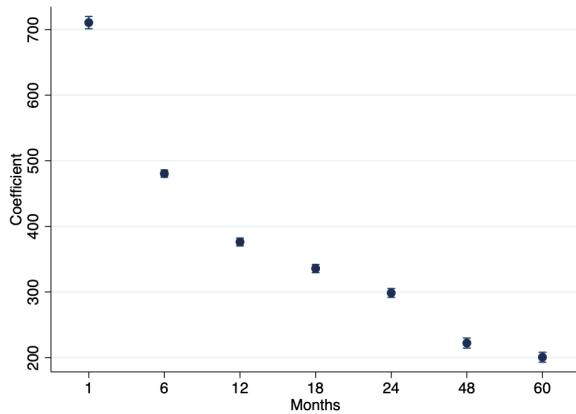
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A Supplementary Figures

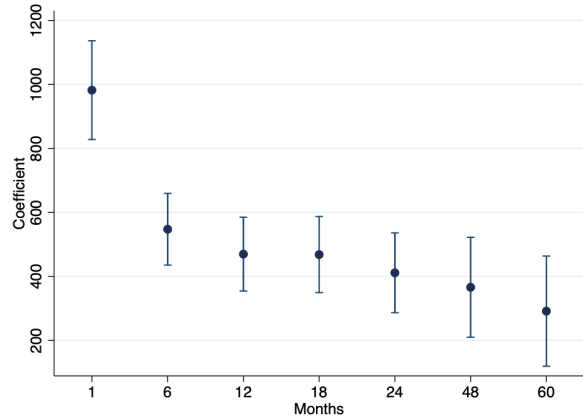
A.1 IV Results

Figure A1: Effect of transitioning into an OEC on earnings

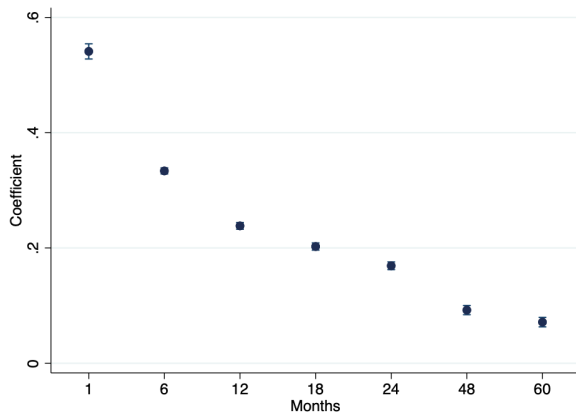
(a) Earnings - OLS



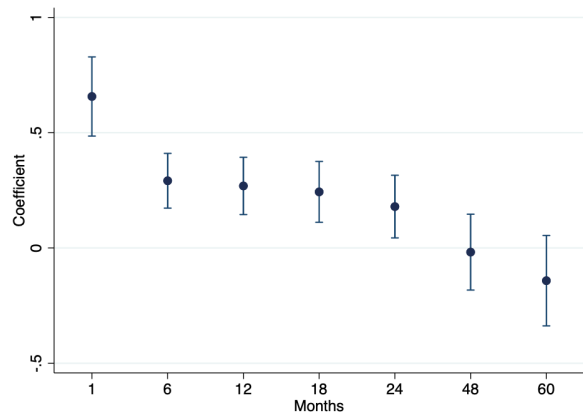
(b) Earnings - IV



(c) Earnings growth



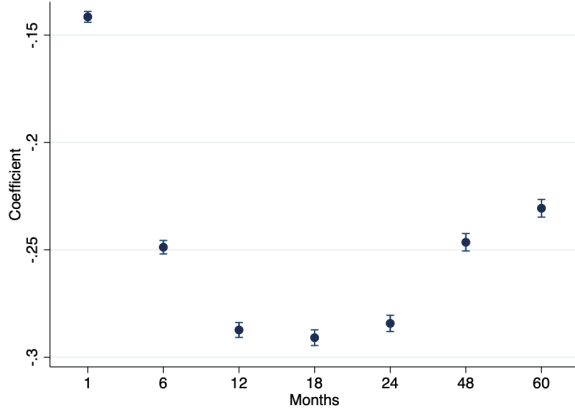
(d) Earnings growth IV



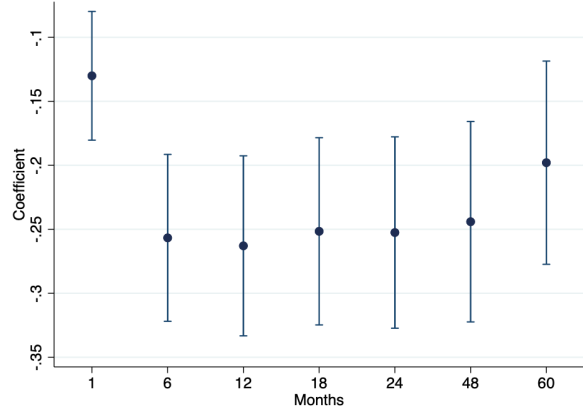
Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t , for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

Figure A2: Effect of transitioning into an OEC on mobility

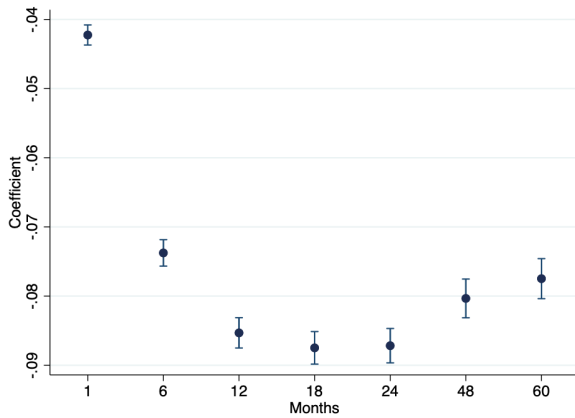
(a) Change Sector - OLS



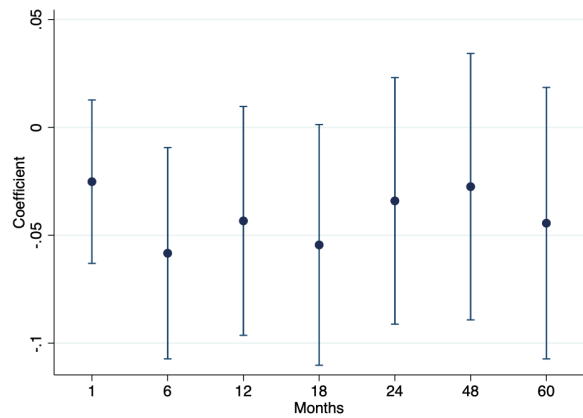
(b) Change Sector - IV



(c) Change Region - OLS

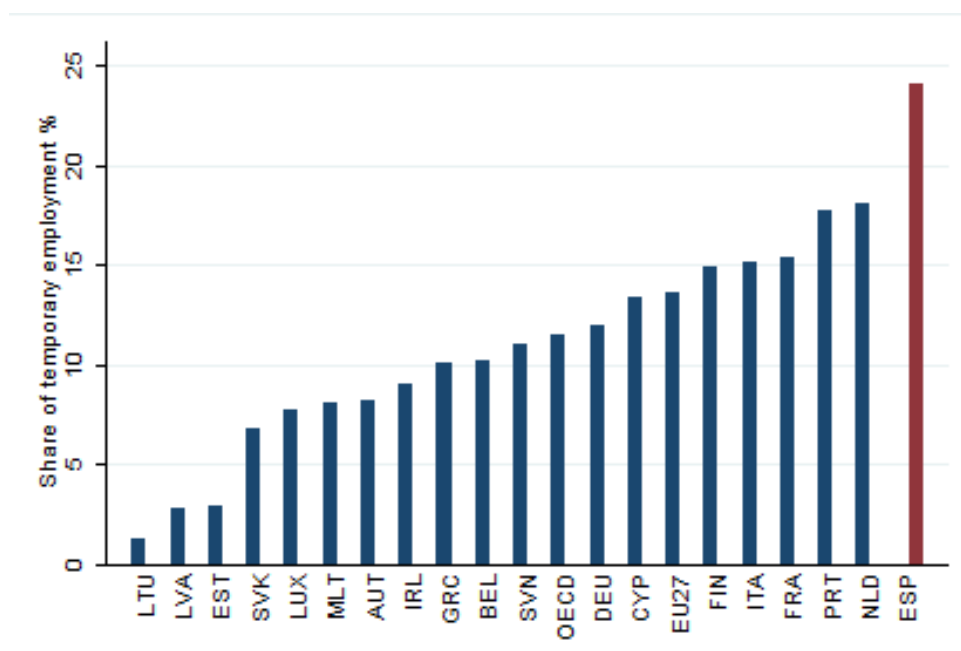


(d) Change Region - IV



Notes: Sample restricted to workers that held a fixed-term position of at least 0.8 but less than 1.2 years of tenure at baseline, who were in the last month of a fixed-term contract between 1998-2012. The coefficients correspond to the effect of the first lead of OEC regional openings on each outcome. All regressions control for the leads and lags of logOEC as well as log of total new contracts. We also control for the mean of the outcomes at time t , for all workers in the unrestricted sample. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, leads and lags of new fixed-term contracts.

Figure A3: Proportion of workers in temporary contracts by country, 2020

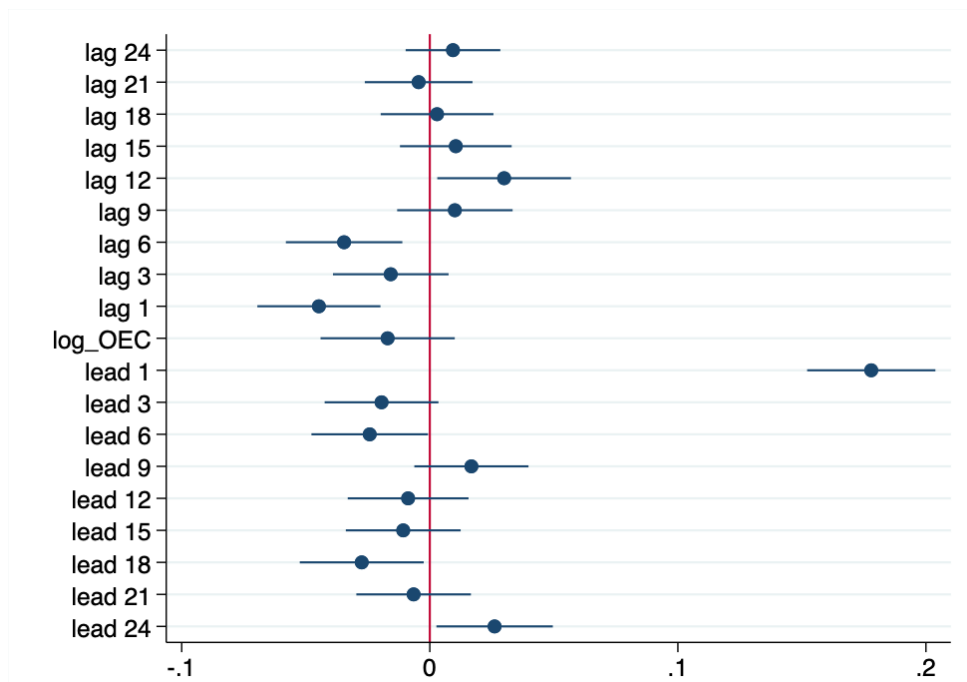


Notes: Temporary employment includes wage and salary workers whose job has a predetermined termination date. This indicator is measured as the percentage of dependent employees (i.e. wage and salary workers).

Source: OECD, Labour Market Statistics: Employment by permanency of the job: incidence

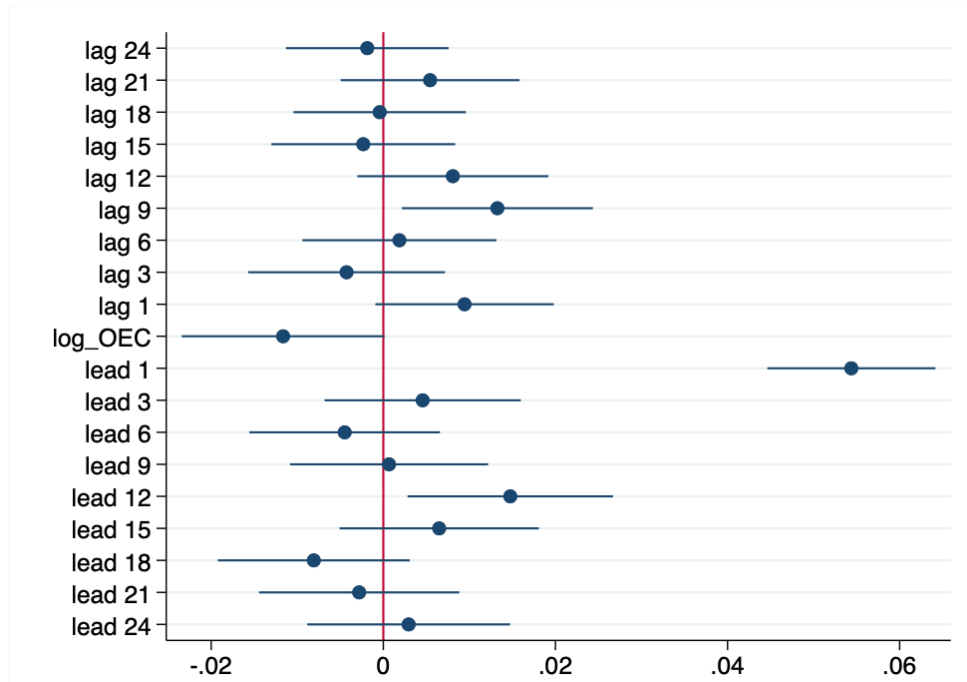
A.2 Selection into permanent positions

Figure A4: National instrument: Promotion probabilities



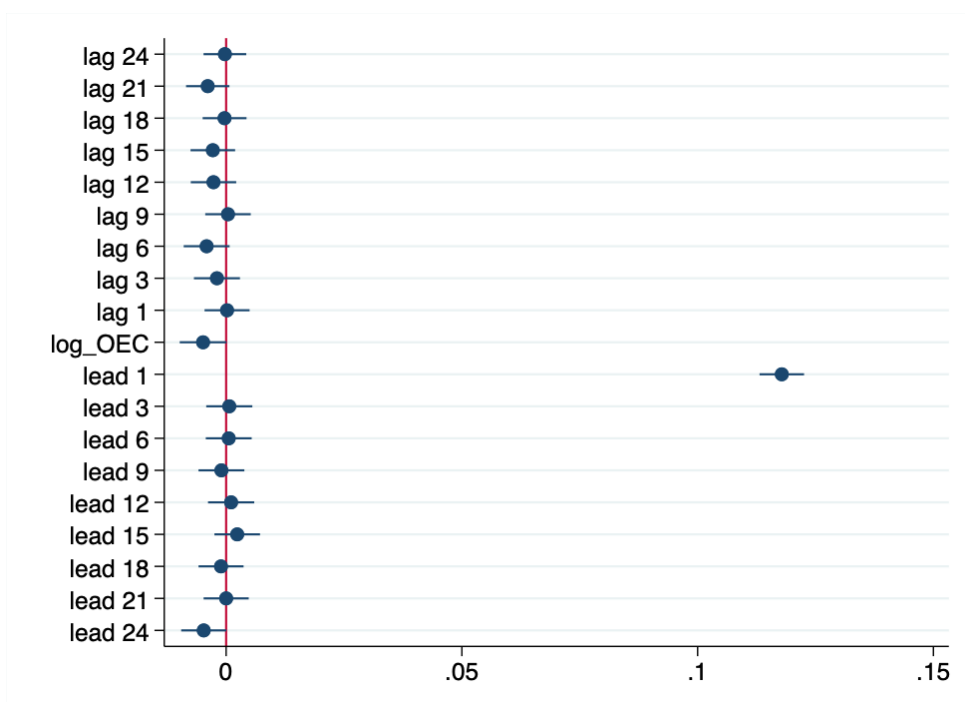
Notes: Sample: Workers in the last month of a fixed-term contract of least 0.8 years of tenure but less than 1.2 years of tenure. Coefficients of the probability of being promoted to a open-ended contract in $t + 1$ on leads and lags of the log of new open ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, foreign born status, interactions of age FE and education attainment, experience, experience squared, and leads and lags of the opening of new fixed-term contracts.

Figure A5: Sectoral instrument: Promotion probabilities



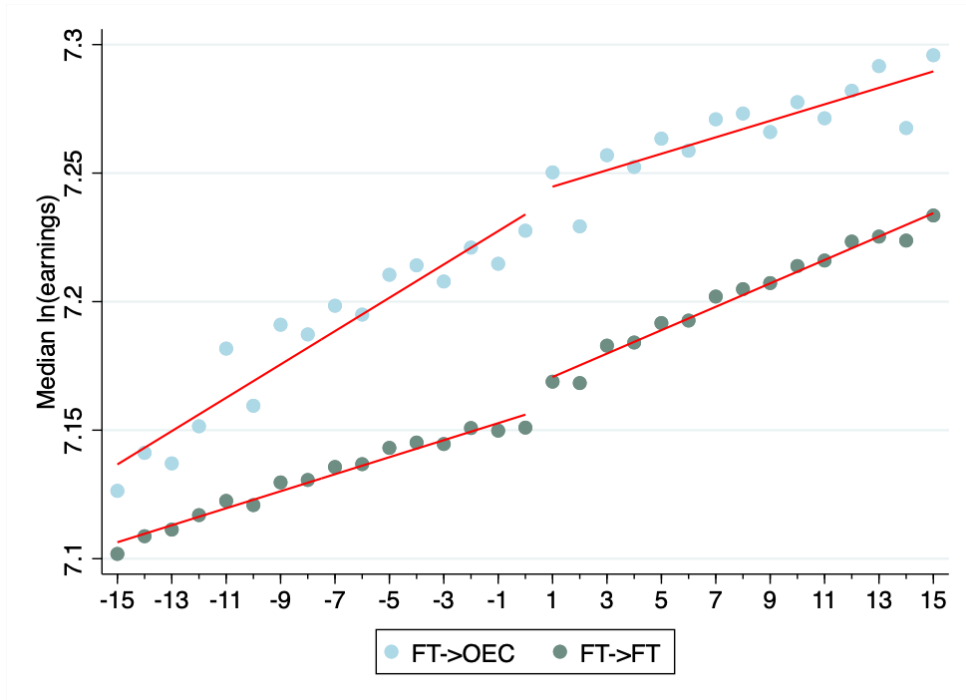
Notes: Sample: Workers in the last month of a fixed-term contract of least 0.8 years of tenure but less than 1.2 years of tenure. Coefficients of the probability of being promoted to an open-ended contract in $t + 1$ on leads and lags of the log of new open-ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, and leads and lags of the opening of new fixed-term contracts.

Figure A6: Regional and sectoral instrument: Promotion probabilities



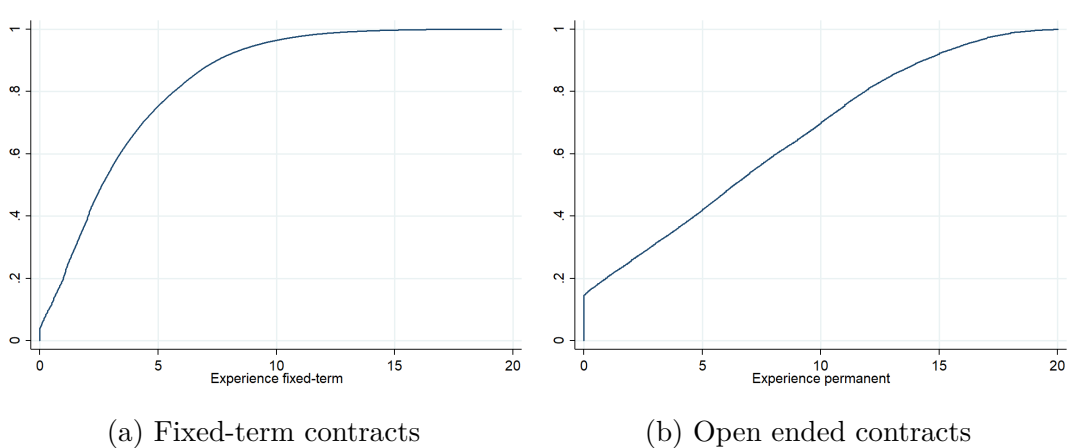
Notes: Sample: Workers in the last month of a fixed-term contract of least 0.8 years of tenure but less than 1.2 years of tenure. Coefficients of the probability of being promoted to an open-ended contract in $t + 1$ on leads and lags of the log of new open-ended contracts by month. Additional controls: year and month FE, province FE, sector FE, gender, foreign-born status, interactions of age FE and education attainment, experience, experience squared, and leads and lags of the opening of new fixed-term contracts.

Figure A7: Evolution of earnings: transitioning to a new contract



Notes: Median log earnings of workers 15 months before and after transitioning to a new contract.

Figure A8: Cumulative distribution of maximum experience per worker



Notes: Maximum experience in the estimation sample by type of contract

B Additional robustness and discussion

B.1 Inequality

The dualism between permanent and fixed-term contracts creates persistent inequalities in the workers' earnings trajectories. The prior evidence establishes that one year of experience can generally have different returns depending on the type of contract where

such experience was acquired. There is a significant share of workers who spend many years on temporary contracts, which has persistent effects on wage distribution.

We study how much of the heterogeneous long-term wage growth can be related to a different cumulative experience in fixed-term and permanent contracts. If experiences in permanent and fixed-term contracts were similarly distributed across young workers, the returns to experiences would not account for much of the variance in realized earnings. However, suppose many workers spend most of their careers on fixed-term contracts while others are just a tiny part. In that case, the returns to experiences could account for a substantial fraction of the variance in realized earnings. By using the sample of workers studied previously, the exercise tracked the variance of earnings and the part of the variance explained by differences in the accumulation of work experience. This exercise follows the approach by [Arellano-Bover and Saltiel \(2021\)](#) and computes:

$$\rho_a = \frac{\text{Var} \left(\sum_{FT, OEC} \hat{\gamma}_m \cdot \text{Exp}(m)_{it} \mid \text{age}_{it} = a \right)}{\text{Var} (\ln y_{it} \mid \text{age}_{it} = a)} \quad \text{and} \quad \rho_a^H = \frac{\text{Var} (\hat{\gamma} \cdot \text{Exp}_{it} \mid \text{age}_{it} = a)}{\text{Var} (\ln y_{it} \mid \text{age}_{it} = a)}$$

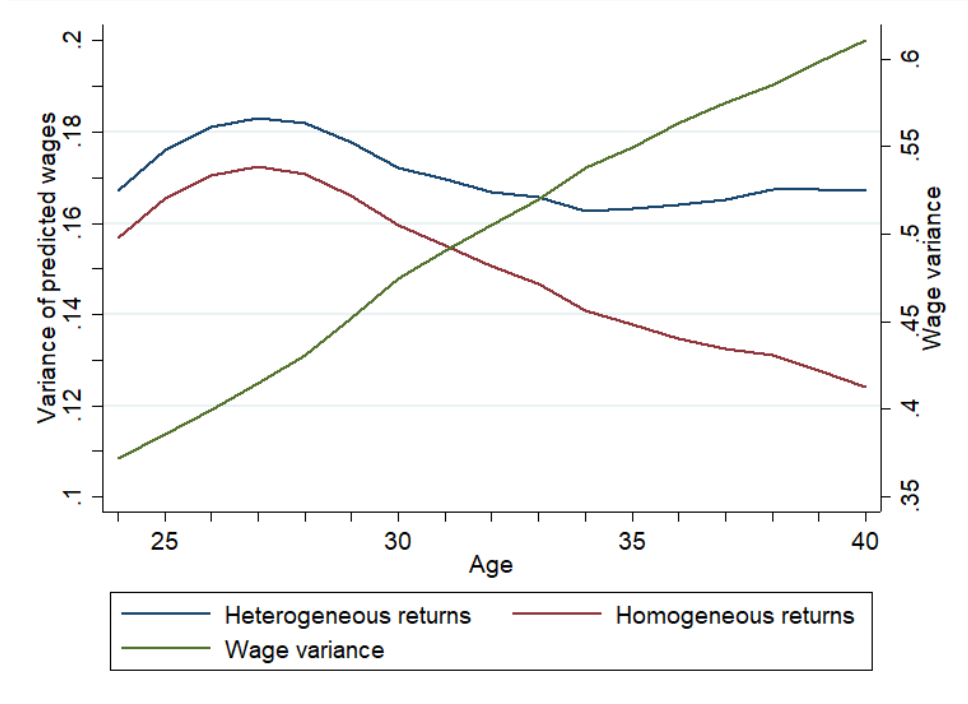
Figure [A9](#) shows the fraction of the variance of wages explained by the returns to experience. The share of earnings variance accounted for by experience decreases in the mid-30s, reaching 16.1% and 13.7% for heterogeneous and homogeneous returns to experience, respectively. After that, the proportion of explained volatility remains stable once the experience quality is considered, assuming homogeneous returns to experience. The explained part continues decreasing. At age 40, the gap in explained earnings volatility is close to 5 p.p. Thus, the conventional approach of assuming all experience to be homogeneous substantially underestimates the fraction of earnings variance accounted for by varying experience profiles across workers.

B.2 Wage growth: Causal impact of fixed-term contracts

The previous results highlight that workers in permanent contracts may experience a smaller wage growth than similar workers. This section studies that directly by investigating how being in a fixed-term contract affects wage growth. Additionally, I implement an AIPW estimator to causally estimate the impact on the wage growth of individuals being hired under a fixed-term contract, conditional on a similar employment history to those hired in permanent contracts.

Consider the following equation:

Figure A9: Variance of returns-to-experiences component over variance of log earnings



Notes: The returns to experience are calculated from a Mincerian equation on experience and interaction of education and gender, education and occupational skill group, age, age squared, sector, province, time fixed effects, and contract type. The homogeneous returns assume the returns to experience are the same regardless of the type of contract—the heterogeneous returns to experience control by the experience in fixed-term contracts and permanent contracts.

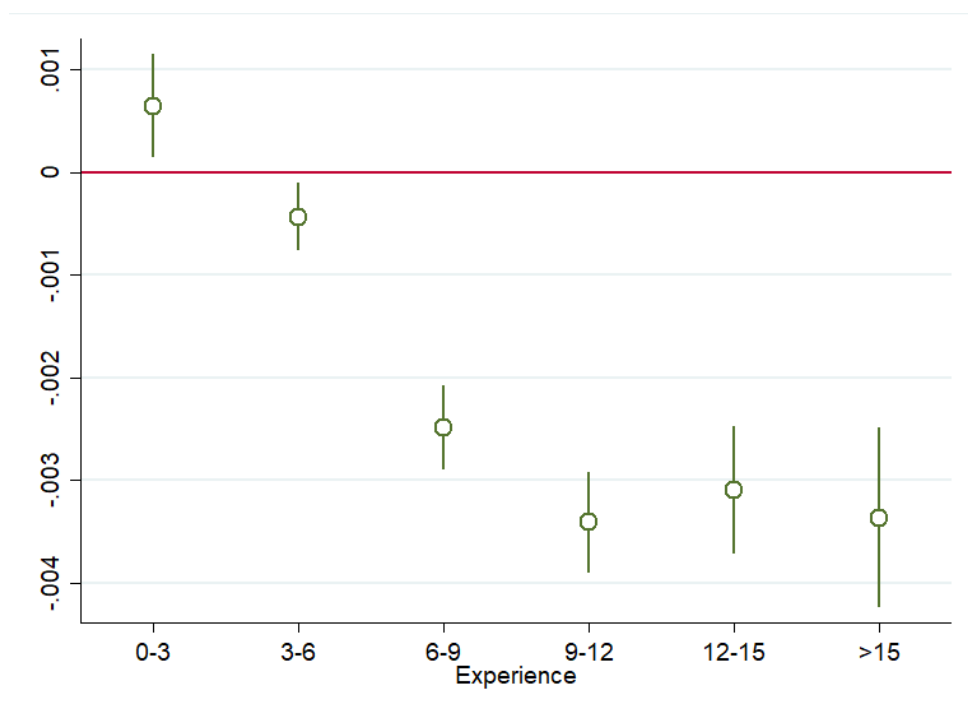
$$\Delta \ln w_{ict} = \text{contract}_{it}^{FT} \gamma + \sigma_c + \psi_t + X_{it} \beta + \varepsilon_{ict}$$

where σ_c is a region fixed effect, ψ_t is a year-month fixed-effect, x_{it} is a vector of time-varying individual and job characteristics, (occupation skill level, education), and ε_{ict} is an error term. This regression is separately estimated at different experience intervals. In particular, at 0-3 years to experience, 3-6, 6-9, 9-12, and more than 15 years of experience.

The results suggest workers have a higher wage growth if being in a fixed-term contract during the first three years of experience. However, it turns negative in the following intervals. Workers get slightly lower wage growth from being employed in a fixed-term contract, which decreases much more at the 9-12 years of labor market experience.

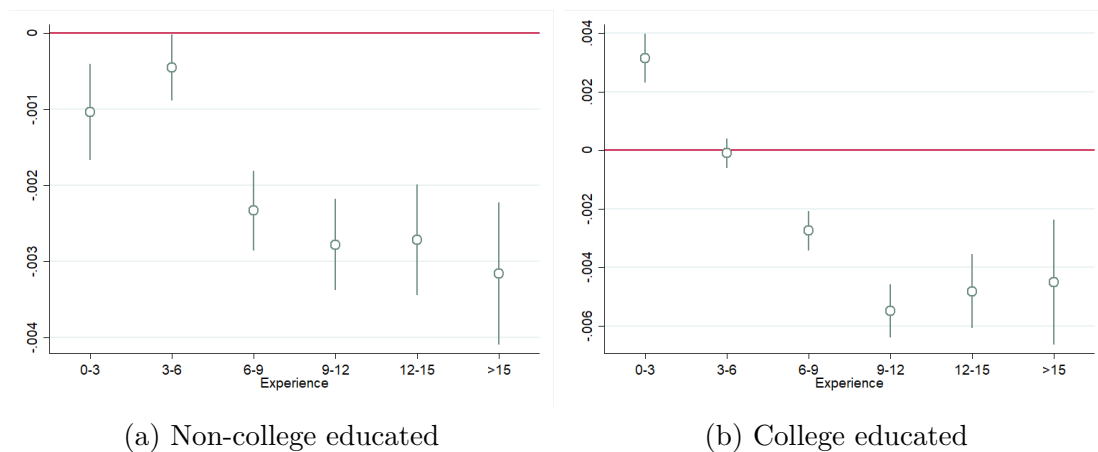
The AIPW estimator has attractive theoretical properties and only requires practitioners to do two things they are already comfortable with: (1) specify a binary regression model for the propensity score and (2) specify a regression model for the outcome variable. Perhaps the most interesting property of this estimator is its so-called double robustness. The estimator remains consistent for the ATE if either the propensity score model or the outcome regression is misspecified, but the other is properly specified.

Figure A10: Gap on returns to experience between fixed-term and open-ended contracts



Notes: The returns to experience are calculated from an IPW design. Controls: gender, education and occupational skill group, age, age squared, sector, province, time fixed effects, and contract type.

Figure A11: Cumulative distribution of maximum experience per worker



(a) Non-college educated

(b) College educated

Notes: Maximum experience in the estimation sample by type of contract

B.3 Job ladder vs human capital

The key insight present in on-the-job search models is that an involuntary unemployment spell cuts a job ladder progression. This is because an unemployed worker looking for a job does not have a current employer as an option to weigh against new offers. In this sense, this brings him to the bottom of the ladder. We categorize workers based on whether they are involuntarily fired and have a period of unemployment larger than four months between one spell and the next. We would expect a pure job ladder mechanism to be unimportant among this group of workers. Hence, evidence of a positive return gap between fixed-term and open-ended contracts would be consistent with a human capital channel or another persistent effect of having more experience as a temporary worker.

We estimate equation 1 using the sub-sample of those experiencing unemployment before the next job and restriction information to the first wage after unemployment. The key takeaway is that we still see similar qualitative effects for this group of workers compared to the baseline estimate. We find evidence a job ladder channel does not fully explain the difference in returns to experience. However, the wage growth of these workers is considerably smaller, which suggests that even though a job ladder mechanism cannot fully explain results, that is very important in explaining wage growth.

Table 3: Laid-off workers: heterogeneous returns to experience by contract type

| | (1) | (2) | (3) |
|---------------------------------|------------------------|------------------------|-------------------------|
| | ln earnings | | |
| <i>experience_{FT}</i> | 0.0168*** (0.00199) | 0.0105*** (0.00189) | 0.00734*** (0.00189) |
| <i>experience_{OEC}</i> | 0.00451* (0.00204) | -0.000514 (0.00197) | 0.00432* (0.00198) |
| <i>exp · exp_{FT}</i> | -0.243 (0.181) | -0.195 (0.171) | -0.147 (0.169) |
| <i>exp · exp_{OEC}</i> | 1.447*** (0.165) | 1.395*** (0.161) | 1.075*** (0.163) |
| Constant | 6.755*** (0.109) | 6.322*** (0.105) | 6.266*** (0.105) |
| Controls | Yes | Yes | Yes |
| Sector | No | Yes | Yes |
| Tenure | No | No | Yes |
| Individual FE | No | No | No |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Every regression controls by worker fixed-effects. Column (1) Baseline specification. Column (2) Sector fixed effects. Column (3) Sector fixed effects and tenure. Column (4) Sector fixed effects, tenure, and type of contract. Standard errors clustered at the individual level. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level.

C Supplementary tables

Table 4: Heterogeneous returns to experience by contract type

| | (1) | (2) | (3) |
|---------------------------------|-------------------------|-------------------------|-------------------------|
| | ln earnings | | |
| experience | 0.0303*** (0.000336) | | |
| <i>experience_{FT}</i> | | 0.0247*** (0.000449) | 0.0266*** (0.000726) |
| <i>experience_{OEC}</i> | | 0.0353*** (0.000380) | 0.0359*** (0.000536) |
| Obs. | 16266496 | 16266496 | 16255262 |
| R2 | 0.474 | 0.482 | 0.751 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Column (1) Baseline specification. Column (2) Considers separately experience in fixed-term and open-ended contracts. Column (3) Additionally includes worker fixed-effects. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level. Regression using shares Source: MCVL 2006-2017

Table 5: Heterogeneous returns to experience by contract type

| | (1) | (2) | (3) | (4) |
|---------------------------------|---------------------------|---------------------------|---------------------------|---------------------------|
| | ln earnings | | | |
| <i>experience_{FT}</i> | 0.0529*** (0.00101) | 0.0529*** (0.00101) | 0.112*** (0.00132) | 0.112*** (0.00132) |
| <i>experience_{OEC}</i> | 0.0542*** (0.000698) | 0.0544*** (0.000697) | 0.0987*** (0.000975) | 0.0987*** (0.000973) |
| <i>exp * exp_{FT}</i> | -2.952*** (0.0821) | -2.919*** (0.0821) | -3.975*** (0.0620) | -3.934*** (0.0619) |
| <i>exp * exp_{OEC}</i> | -0.961*** (0.0411) | -0.984*** (0.0410) | -1.714*** (0.0298) | -1.735*** (0.0297) |
| contract fixed-term | -0.0235*** (0.00155) | -0.0248*** (0.00155) | -0.0348*** (0.00113) | -0.0358*** (0.00113) |
| tenure | -0.00242*** (0.000411) | -0.00233*** (0.000411) | -0.00432*** (0.000246) | -0.00414*** (0.000245) |
| Constant | 8.379*** (0.0140) | 8.060*** (0.0215) | 7.411*** (0.0155) | 7.099*** (0.0208) |
| Controls | Yes | Yes | Yes | Yes |
| Individual FE | No | No | Yes | Yes |
| Sector | share | share | share | share |
| Tenure | Yes | Yes | Yes | Yes |
| Skill | FE | FE & Share | FE | FE & Share |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers who entered the labor market between 1998 and 2003. Workers aged 18-40 years old. Every regression controls by worker fixed-effects. Column (1) Baseline specification. Column (2) Sector fixed effects. Column (3) Sector fixed effects and tenure. Column (4) Sector fixed effects, tenure, and type of contract. Standard errors clustered at the individual level. Additional controls: time and region fixed effects, interactions of gender with educational level, and interactions of occupation skill group and educational level. Regression using shares

Table 6: Heterogeneous returns to experience by contract type

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | ln earnings | | | | |
| <i>experience</i> | 0.0508*** (0.000528) | | | | |
| <i>experience</i> ² | -1.314*** (0.0323) | | | | |
| <i>exp_{FT}</i> | | 0.0551*** (0.000788) | 0.0637*** (0.000795) | 0.0794*** (0.00103) | 0.0723*** (0.00105) |
| <i>exp_{OEC}</i> | | 0.0539*** (0.000630) | 0.0558*** (0.000613) | 0.0706*** (0.000712) | 0.0670*** (0.000759) |
| <i>exp</i> × <i>exp_{FT}</i> | | -2.454*** (0.0636) | -3.373*** (0.0634) | -3.312*** (0.0552) | -2.398*** (0.0554) |
| <i>exp</i> × <i>exp_{OEC}</i> | | -0.975*** (0.0401) | -1.049*** (0.0389) | -1.446*** (0.0311) | -1.298*** (0.0323) |
| Obs. | 16266496 | 16266496 | 16266496 | 16255262 | 16255262 |
| <i>R</i> ² | 0.475 | 0.484 | 0.478 | 0.754 | 0.758 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: *exp*, *exp_{FT}*, and *exp_{OEC}* account for experience, experience in fixed-term, and experience in open-ended contracts, respectively. Controls include gender and occupation-skill group interactions on education attainment, sector, region and time fixed-effects, age, age squared, and interactions of tenure with an indicator for a fixed-term contract. Errors are clustered at the worker level. Column (2) and (4) includes interactions of education with age categories

Table 7: National instrument: Baseline specification

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|-----------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | Promotion to OEC in $t + 1$ | | | | |
| $\log OEC_{12}^{lag}$ | 0.0526*** (0.00486) | -0.0212** (0.00655) | -0.0228*** (0.00650) | -0.0191** (0.00637) | -0.0182** (0.00633) |
| $\log OEC_{11}^{lag}$ | 0.0570*** (0.00712) | 0.0799*** (0.00822) | 0.0801*** (0.00813) | 0.0705*** (0.00786) | 0.0712*** (0.00783) |
| $\log OEC_{10}^{lag}$ | 0.0299*** (0.00824) | 0.0249** (0.00890) | 0.0266** (0.00878) | 0.0209* (0.00852) | 0.0214* (0.00846) |
| $\log OEC_9^{lag}$ | 0.0246** (0.00815) | 0.00553 (0.00874) | 0.00286 (0.00863) | -0.000307 (0.00844) | -0.000491 (0.00838) |
| $\log OEC_8^{lag}$ | -0.0244** (0.00827) | -0.0130 (0.00865) | -0.0127 (0.00855) | -0.00972 (0.00835) | -0.0103 (0.00829) |
| $\log OEC_7^{lag}$ | -0.0131 (0.00855) | 0.0445*** (0.00910) | 0.0448*** (0.00899) | 0.0399*** (0.00878) | 0.0398*** (0.00871) |
| $\log OEC_6^{lag}$ | 0.0510*** (0.00831) | 0.0195* (0.00882) | 0.0192* (0.00871) | 0.0151 (0.00844) | 0.0136 (0.00839) |
| $\log OEC_5^{lag}$ | -0.0515*** (0.00851) | -0.0281** (0.00900) | -0.0282** (0.00889) | -0.0277** (0.00864) | -0.0277** (0.00858) |
| $\log OEC_4^{lag}$ | -0.0508*** (0.00858) | -0.0503*** (0.00915) | -0.0487*** (0.00904) | -0.0448*** (0.00882) | -0.0451*** (0.00876) |
| $\log OEC_3^{lag}$ | 0.0265** (0.00865) | 0.0122 (0.00943) | 0.00896 (0.00932) | 0.00963 (0.00908) | 0.0127 (0.00902) |
| $\log OEC_2^{lag}$ | -0.0691*** (0.00857) | -0.0419*** (0.00941) | -0.0419*** (0.00930) | -0.0371*** (0.00904) | -0.0346*** (0.00898) |
| $\log OEC_1^{lag}$ | -0.0272** (0.00872) | -0.0324*** (0.00955) | -0.0288** (0.00944) | -0.0239** (0.00918) | -0.0212* (0.00912) |
| $\log OEC_0$ | -0.0479*** (0.00905) | -0.0551*** (0.0100) | -0.0542*** (0.00991) | -0.0444*** (0.00964) | -0.0437*** (0.00959) |
| $\log OEC_1^{lead}$ | 0.115*** (0.00771) | 0.267*** (0.00944) | 0.260*** (0.00935) | 0.247*** (0.00907) | 0.242*** (0.00902) |
| $\log OEC_2^{lead}$ | 0.0481*** (0.00872) | 0.0283** (0.0107) | 0.0253* (0.0106) | 0.0196 (0.0103) | 0.0168 (0.0102) |
| $\log OEC_3^{lead}$ | -0.00237 (0.00864) | -0.0252* (0.0103) | -0.0220* (0.0102) | -0.0188 (0.00994) | -0.0188 (0.00988) |
| $\log OEC_4^{lead}$ | 0.0346*** (0.00878) | 0.00872 (0.0100) | 0.0112 (0.00995) | 0.00808 (0.00975) | 0.00931 (0.00970) |
| $\log OEC_5^{lead}$ | -0.0335*** (0.00901) | 0.00845 (0.0104) | 0.00958 (0.0103) | 0.0120 (0.0100) | 0.0145 (0.00999) |
| $\log OEC_6^{lead}$ | -0.0808*** (0.00880) | -0.0431*** (0.0101) | -0.0461*** (0.0100) | -0.0370*** (0.00974) | -0.0346*** (0.00969) |
| $\log OEC_7^{lead}$ | 0.0566*** (0.00892) | 0.0565*** (0.0104) | 0.0597*** (0.0103) | 0.0561*** (0.0100) | 0.0544*** (0.00996) |
| $\log OEC_8^{lead}$ | 0.00894 (0.00914) | -0.0371*** (0.0106) | -0.0366*** (0.0105) | -0.0356*** (0.0103) | -0.0343*** (0.0102) |
| $\log OEC_9^{lead}$ | -0.0353*** (0.00900) | 0.00484 (0.0108) | 0.00220 (0.0107) | 0.00474 (0.0104) | 0.00269 (0.0103) |
| $\log OEC_{10}^{lead}$ | 0.0666*** (0.00917) | 0.00441 (0.0110) | 0.00680 (0.0109) | 0.000621 (0.0106) | -0.000503 (0.0105) |
| $\log OEC_{11}^{lead}$ | 0.0445*** (0.00904) | -0.0332** (0.0112) | -0.0370*** (0.0110) | -0.0348** (0.0107) | -0.0372*** (0.0107) |
| $\log OEC_{12}^{lead}$ | 0.0196* (0.00837) | -0.0140 (0.0117) | -0.0142 (0.0116) | -0.0166 (0.0113) | -0.0144 (0.0112) |
| Obs. | 331,467 | 331,467 | 331,467 | 331,467 | 331,467 |
| R2 | 0.027 | 0.036 | 0.061 | 0.115 | 0.126 |
| Time FE | No | Yes | Yes | Yes | Yes |
| Region FE | No | No | Yes | Yes | Yes |
| Sector FE | No | No | No | Yes | Yes |
| Individual FE | No | No | No | No | Yes |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 8: National instrument: Control by new FT contracts

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|-----------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | Promotion to OEC in $t + 1$ | | | | |
| $\log OEC_{12}^{lag}$ | 0.0707*** (0.00543) | -0.0343*** (0.00763) | -0.0333*** (0.00756) | -0.0291*** (0.00741) | -0.0282*** (0.00736) |
| $\log OEC_{11}^{lag}$ | 0.0844*** (0.00782) | 0.108*** (0.00993) | 0.108*** (0.00982) | 0.0983*** (0.00950) | 0.100*** (0.00946) |
| $\log OEC_{10}^{lag}$ | 0.00936 (0.00902) | 0.0106 (0.0103) | 0.0129 (0.0102) | 0.00978 (0.00984) | 0.0106 (0.00978) |
| $\log OEC_9^{lag}$ | 0.00405 (0.00867) | 0.00903 (0.00978) | 0.00837 (0.00966) | 0.00552 (0.00944) | 0.00615 (0.00937) |
| $\log OEC_8^{lag}$ | -0.0245** (0.00879) | -0.00208 (0.00958) | -0.00121 (0.00948) | 0.00119 (0.00925) | 0.00109 (0.00919) |
| $\log OEC_7^{lag}$ | 0.0270** (0.00920) | 0.0389*** (0.0100) | 0.0423*** (0.00993) | 0.0384*** (0.00970) | 0.0386*** (0.00961) |
| $\log OEC_6^{lag}$ | 0.0378*** (0.00898) | 0.0252** (0.00972) | 0.0244* (0.00961) | 0.0185* (0.00932) | 0.0170 (0.00926) |
| $\log OEC_5^{lag}$ | -0.0358*** (0.00909) | -0.0135 (0.0101) | -0.0134 (0.00996) | -0.0145 (0.00968) | -0.0144 (0.00962) |
| $\log OEC_4^{lag}$ | -0.0270** (0.00924) | -0.0495*** (0.00991) | -0.0500*** (0.00979) | -0.0485*** (0.00955) | -0.0493*** (0.00948) |
| $\log OEC_3^{lag}$ | 0.0365*** (0.00931) | 0.00636 (0.0104) | 0.00416 (0.0103) | 0.00506 (0.0100) | 0.00827 (0.00996) |
| $\log OEC_2^{lag}$ | -0.0373*** (0.00905) | -0.0504*** (0.0103) | -0.0503*** (0.0102) | -0.0454*** (0.00987) | -0.0422*** (0.00980) |
| $\log OEC_1^{lag}$ | -0.0370*** (0.00917) | -0.0278** (0.0105) | -0.0259* (0.0104) | -0.0230* (0.0101) | -0.0199* (0.0100) |
| $\log_0 EC_0$ | -0.0592*** (0.00954) | -0.0395*** (0.0115) | -0.0439*** (0.0114) | -0.0382*** (0.0111) | -0.0365*** (0.0110) |
| $\log OEC_1^{lead}$ | 0.179*** (0.00891) | 0.264*** (0.0112) | 0.253*** (0.0111) | 0.236*** (0.0107) | 0.231*** (0.0107) |
| $\log OEC_2^{lead}$ | 0.0639*** (0.0103) | 0.0428*** (0.0122) | 0.0383** (0.0121) | 0.0280* (0.0117) | 0.0259* (0.0117) |
| $\log OEC_3^{lead}$ | -0.0595*** (0.00981) | -0.0334** (0.0115) | -0.0348** (0.0114) | -0.0341** (0.0111) | -0.0338** (0.0111) |
| $\log OEC_4^{lead}$ | -0.00244 (0.00993) | 0.0182 (0.0114) | 0.0188 (0.0113) | 0.0108 (0.0111) | 0.0128 (0.0110) |
| $\log OEC_5^{lead}$ | -0.0371*** (0.0102) | 0.00316 (0.0118) | -0.000223 (0.0117) | -0.00246 (0.0114) | 0.000363 (0.0113) |
| $\log OEC_6^{lead}$ | -0.0348*** (0.0100) | -0.00952 (0.0114) | -0.0157 (0.0113) | -0.0133 (0.0109) | -0.0116 (0.0109) |
| $\log OEC_7^{lead}$ | 0.0233* (0.0102) | 0.0562*** (0.0122) | 0.0537*** (0.0120) | 0.0464*** (0.0117) | 0.0441*** (0.0116) |
| $\log OEC_8^{lead}$ | 0.0151 (0.0104) | -0.0306** (0.0116) | -0.0333** (0.0115) | -0.0336** (0.0112) | -0.0323** (0.0112) |
| $\log OEC_9^{lead}$ | -0.0801*** (0.0103) | 0.0212 (0.0123) | 0.0151 (0.0121) | 0.0146 (0.0118) | 0.0121 (0.0118) |
| $\log OEC_{10}^{lead}$ | -0.00803 (0.0105) | 0.00726 (0.0119) | 0.00753 (0.0118) | -0.0000973 (0.0115) | -0.00112 (0.0114) |
| $\log OEC_{11}^{lead}$ | -0.0201 (0.0103) | -0.00958 (0.0123) | -0.0165 (0.0121) | -0.0185 (0.0118) | -0.0203 (0.0117) |
| $\log OEC_{12}^{lead}$ | 0.00611 (0.00992) | -0.0131 (0.0133) | -0.0127 (0.0131) | -0.0149 (0.0127) | -0.0125 (0.0127) |
| Obs. | 331,467 | 331,467 | 331,467 | 331,467 | 331,467 |
| R2 | 0.033 | 0.037 | 0.062 | 0.115 | 0.126 |
| Time FE | No | Yes | Yes | Yes | Yes |
| Region FE | No | No | Yes | Yes | Yes |
| Sector FE | No | No | No | Yes | Yes |
| Individual FE | No | No | No | No | Yes |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 9: Regional instrument: Baseline specification

| | (1) | (2) | (3) | (4) | (5) |
|-----------------------|-----------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | Promotion to OEC in $t + 1$ | | | | |
| $\log OEC_{12}^{lag}$ | 0.00628 (0.00321) | -0.00202 (0.00362) | -0.00192 (0.00361) | 0.000687 (0.00351) | 0.00111 (0.00349) |
| $\log OEC_{11}^{lag}$ | 0.0164*** (0.00347) | 0.0645*** (0.00384) | 0.0623*** (0.00381) | 0.0582*** (0.00369) | 0.0588*** (0.00367) |
| $\log OEC_{10}^{lag}$ | 0.0246*** (0.00357) | 0.0107** (0.00400) | 0.00910* (0.00397) | 0.00690 (0.00384) | 0.00760* (0.00381) |
| $\log OEC_9^{lag}$ | -0.00454 (0.00357) | -0.00578 (0.00395) | -0.00580 (0.00393) | -0.00423 (0.00381) | -0.00379 (0.00379) |
| $\log OEC_8^{lag}$ | -0.00470 (0.00370) | 0.00464 (0.00406) | 0.00345 (0.00402) | 0.00523 (0.00390) | 0.00495 (0.00387) |
| $\log OEC_7^{lag}$ | -0.0181*** (0.00370) | 0.0205*** (0.00409) | 0.0163*** (0.00406) | 0.0162*** (0.00394) | 0.0165*** (0.00391) |
| $\log OEC_6^{lag}$ | -0.00562 (0.00365) | 0.0171*** (0.00405) | 0.0160*** (0.00400) | 0.0124** (0.00388) | 0.0128*** (0.00385) |
| $\log OEC_5^{lag}$ | -0.0131*** (0.00372) | 0.00357 (0.00410) | -0.000638 (0.00406) | -0.00207 (0.00394) | -0.00223 (0.00392) |
| $\log OEC_4^{lag}$ | -0.0252*** (0.00369) | -0.000847 (0.00410) | -0.00826* (0.00407) | -0.00840* (0.00394) | -0.00868* (0.00391) |
| $\log OEC_3^{lag}$ | 0.00431 (0.00371) | 0.0301*** (0.00415) | 0.0262*** (0.00410) | 0.0256*** (0.00397) | 0.0261*** (0.00395) |
| $\log OEC_2^{lag}$ | 0.00502 (0.00370) | 0.0115** (0.00415) | 0.00326 (0.00411) | 0.00451 (0.00398) | 0.00610 (0.00396) |
| $\log OEC_1^{lag}$ | 0.0204*** (0.00372) | 0.0218*** (0.00416) | 0.0165*** (0.00413) | 0.0181*** (0.00400) | 0.0186*** (0.00397) |
| $\log OEC_0$ | 0.0213*** (0.00396) | 0.00797 (0.00422) | 0.00601 (0.00418) | 0.00670 (0.00405) | 0.00607 (0.00403) |
| $\log OEC_1^{lead}$ | 0.114*** (0.00370) | 0.138*** (0.00414) | 0.135*** (0.00409) | 0.127*** (0.00395) | 0.125*** (0.00393) |
| $\log OEC_2^{lead}$ | 0.00571 (0.00379) | -0.0437*** (0.00433) | -0.0464*** (0.00428) | -0.0412*** (0.00413) | -0.0413*** (0.00411) |
| $\log OEC_3^{lead}$ | 0.000147 (0.00376) | -0.0374*** (0.00425) | -0.0386*** (0.00421) | -0.0333*** (0.00407) | -0.0322*** (0.00405) |
| $\log OEC_4^{lead}$ | 0.00700 (0.00385) | -0.0174*** (0.00431) | -0.0216*** (0.00426) | -0.0210*** (0.00413) | -0.0202*** (0.00410) |
| $\log OEC_5^{lead}$ | -0.0139*** (0.00384) | -0.00606 (0.00434) | -0.0136** (0.00429) | -0.0146*** (0.00415) | -0.0134** (0.00412) |
| $\log OEC_6^{lead}$ | -0.0148*** (0.00380) | -0.0155*** (0.00426) | -0.0230*** (0.00421) | -0.0232*** (0.00408) | -0.0231*** (0.00406) |
| $\log OEC_7^{lead}$ | 0.00476 (0.00385) | -0.0122** (0.00432) | -0.0176*** (0.00427) | -0.0194*** (0.00414) | -0.0186*** (0.00412) |
| $\log OEC_8^{lead}$ | -0.0269*** (0.00383) | -0.0217*** (0.00431) | -0.0277*** (0.00426) | -0.0263*** (0.00412) | -0.0256*** (0.00409) |
| $\log OEC_9^{lead}$ | -0.0167*** (0.00379) | -0.0178*** (0.00428) | -0.0258*** (0.00425) | -0.0233*** (0.00411) | -0.0228*** (0.00408) |
| $\log OEC_1^{lead}0$ | -0.0124** (0.00380) | -0.0353*** (0.00433) | -0.0412*** (0.00431) | -0.0387*** (0.00417) | -0.0389*** (0.00414) |
| $\log OEC_1^{lead}1$ | -0.00580 (0.00373) | -0.0319*** (0.00429) | -0.0371*** (0.00427) | -0.0330*** (0.00414) | -0.0320*** (0.00411) |
| $\log OEC_1^{lead}2$ | -0.0286*** (0.00373) | -0.0459*** (0.00426) | -0.0493*** (0.00426) | -0.0439*** (0.00412) | -0.0418*** (0.00410) |
| Obs. | 331,032 | 331,032 | 331,032 | 331,032 | 331,032 |
| R2 | 0.030 | 0.043 | 0.060 | 0.114 | 0.125 |
| Time FE | No | Yes | Yes | Yes | Yes |
| Region FE | No | No | Yes | Yes | Yes |
| Sector FE | No | No | No | Yes | Yes |
| Individual FE | No | No | No | No | Yes |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017

Table 10: Regional instrument: Control by new FT contracts

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|-----------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | Promotion to OEC in $t + 1$ | | | | |
| $\log OEC_{12}^{lag}$ | 0.0144*** (0.00336) | 0.00616 (0.00366) | 0.00270 (0.00367) | 0.00466 (0.00357) | 0.00496 (0.00355) |
| $\log OEC_{11}^{lag}$ | 0.0415*** (0.00359) | 0.0790*** (0.00387) | 0.0708*** (0.00386) | 0.0663*** (0.00374) | 0.0668*** (0.00371) |
| $\log OEC_{10}^{lag}$ | 0.0218*** (0.00376) | 0.0175*** (0.00404) | 0.0125** (0.00403) | 0.00999* (0.00390) | 0.0107** (0.00387) |
| $\log OEC_9^{lag}$ | -0.00666 (0.00373) | 0.000724 (0.00399) | -0.00652 (0.00398) | -0.00508 (0.00386) | -0.00451 (0.00383) |
| $\log OEC_8^{lag}$ | -0.00214 (0.00385) | 0.0111** (0.00408) | 0.00499 (0.00407) | 0.00652 (0.00395) | 0.00647 (0.00392) |
| $\log OEC_7^{lag}$ | -0.00266 (0.00385) | 0.0216*** (0.00414) | 0.0144*** (0.00412) | 0.0145*** (0.00400) | 0.0148*** (0.00397) |
| $\log OEC_6^{lag}$ | 0.00964* (0.00379) | 0.0190*** (0.00408) | 0.0155*** (0.00406) | 0.0123** (0.00393) | 0.0126** (0.00391) |
| $\log OEC_5^{lag}$ | -0.0108** (0.00386) | 0.00783 (0.00413) | 0.000992 (0.00412) | 0.000277 (0.00400) | -0.0000216 (0.00397) |
| $\log OEC_4^{lag}$ | -0.000633 (0.00386) | -0.000509 (0.00414) | -0.00928* (0.00413) | -0.00898* (0.00400) | -0.00959* (0.00397) |
| $\log OEC_3^{lag}$ | 0.0134*** (0.00388) | 0.0351*** (0.00417) | 0.0277*** (0.00415) | 0.0265*** (0.00403) | 0.0268*** (0.00400) |
| $\log OEC_2^{lag}$ | 0.0133*** (0.00386) | 0.0185*** (0.00418) | 0.00550 (0.00418) | 0.00620 (0.00405) | 0.00757 (0.00402) |
| $\log OEC_1^{lag}$ | 0.0199*** (0.00388) | 0.0278*** (0.00419) | 0.0181*** (0.00418) | 0.0187*** (0.00405) | 0.0191*** (0.00402) |
| $\log OEC_0$ | 0.0258*** (0.00405) | 0.0140*** (0.00424) | 0.00698 (0.00423) | 0.00682 (0.00409) | 0.00628 (0.00407) |
| $\log OEC_1^{lead}$ | 0.135*** (0.00385) | 0.144*** (0.00417) | 0.133*** (0.00415) | 0.125*** (0.00401) | 0.122*** (0.00399) |
| $\log OEC_2^{lead}$ | -0.00344 (0.00400) | -0.0405*** (0.00436) | -0.0504*** (0.00435) | -0.0457*** (0.00421) | -0.0457*** (0.00418) |
| $\log OEC_3^{lead}$ | -0.0138*** (0.00396) | -0.0355*** (0.00428) | -0.0474*** (0.00428) | -0.0422*** (0.00414) | -0.0409*** (0.00412) |
| $\log OEC_4^{lead}$ | -0.00377 (0.00403) | -0.0156*** (0.00433) | -0.0281*** (0.00433) | -0.0276*** (0.00419) | -0.0265*** (0.00416) |
| $\log OEC_5^{lead}$ | -0.0111** (0.00405) | -0.00878* (0.00437) | -0.0228*** (0.00435) | -0.0236*** (0.00421) | -0.0223*** (0.00418) |
| $\log OEC_6^{lead}$ | -0.00389 (0.00400) | -0.0163*** (0.00430) | -0.0298*** (0.00429) | -0.0295*** (0.00415) | -0.0293*** (0.00413) |
| $\log OEC_7^{lead}$ | -0.00202 (0.00407) | -0.00971* (0.00435) | -0.0238*** (0.00434) | -0.0249*** (0.00421) | -0.0242*** (0.00419) |
| $\log OEC_8^{lead}$ | -0.00665 (0.00403) | -0.0176*** (0.00433) | -0.0308*** (0.00433) | -0.0291*** (0.00418) | -0.0287*** (0.00416) |
| $\log OEC_9^{lead}$ | -0.0153*** (0.00402) | -0.0125** (0.00431) | -0.0279*** (0.00432) | -0.0262*** (0.00417) | -0.0258*** (0.00414) |
| $\log OEC_{10}^{lead}$ | -0.0129** (0.00407) | -0.0240*** (0.00436) | -0.0404*** (0.00438) | -0.0385*** (0.00424) | -0.0389*** (0.00421) |
| $\log OEC_{11}^{lead}$ | -0.00684 (0.00399) | -0.0191*** (0.00433) | -0.0344*** (0.00435) | -0.0315*** (0.00421) | -0.0304*** (0.00419) |
| $\log OEC_{12}^{lead}$ | -0.0199*** (0.00395) | -0.0319*** (0.00431) | -0.0467*** (0.00434) | -0.0423*** (0.00420) | -0.0402*** (0.00417) |
| Obs. | 331,032 | 331,032 | 331,032 | 331,032 | 331,032 |
| R2 | 0.042 | 0.052 | 0.061 | 0.114 | 0.125 |
| Time FE | No | Yes | Yes | Yes | Yes |
| Region FE | No | No | Yes | Yes | Yes |
| Sector FE | No | No | No | Yes | Yes |
| Individual FE | No | No | No | No | Yes |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Sample: Workers in the last month of a fixed-term contract with tenure of at least 2/3 of a year. Outcome variable if the individual is promoted to OEC in $t + 1$ Column (1) controls for leads and lags of new OEC and FTC. Column (2) adds year and month. Column (3) adds province FE. Column (4) adds sector FE. Column (5) adds gender, foreign born status, interaction of age FE and education attainment, experience, and experience squared. Source: MCVL 2006-2017